

ANCIENT CITIES OF THE INDUS

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Edited by

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TO
CHRISTINE AND MICHAEL

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Contents

Part VI The Major Settlements of the Indus Civilization

Part VII The Environment

Part VIII The Indus Script

Part IX The Post-urban Phase of the Indus Civilization

Part X The Radiocarbon Chronology

Part XI An Extensive Bibliography of the Indus Civilization including References Cited in the Text

Introduction

My study of the Indus Civilization has brought to light a body of excellent literature on various aspects of this ancient urban complex which has not been incorporated into the three or four standard sources frequently cited in secondary literature. Most of this supplementary material is in journals and other periodicals, often difficult to locate even in the best libraries. Yet, it constitutes an essential source for a comprehensive study of this civilization. To bring some of this literature together within a single volume was a major motivation behind the preparation of this book. An attempt to correct a certain imbalance in our understanding of this civilization is of equal importance. Too often it has been the standard source, especially Sir Mortimer Wheeler's *The Indus Civilization* (1968), to which scholars have turned for information and interpretation. There is no doubt about the excellence of Wheeler's book, but it is still largely a reflection of his perspective. In a sense, then, *Ancient Cities of the Indus* was compiled in an effort to introduce a range of opinion and interpretation of issues too often considered settled.

The reader will sense in many of the papers, which follow this Introduction, a search for paradigm rather than concise, final answers to the problems facing those who study the Indus Civilization. This can be attributed to several factors. First, the synthesis of the field data gathered in the twenties and thirties at Mohenjo daro, Harappa and Chanhu daro was largely complete by the end of the fifties through the efforts of Sir John Marshall, E.J.H. Mackay, Sir Mortimer Wheeler and Stuart Piggott. Many of their interpretations have now been challenged by others. For example, both the widely referenced hypothesis concerning the Aryan destruction of the cities and the notion of twin capitals ruling a vast empire have been largely laid to rest by more recent scholarship. These critiques have not, however, replaced the earlier interpretations with fully viable alternatives. It must also be considered that, with the creation of Pakistan in 1947, there was renewed excavation in the Indus Valley at the important sites of Kot Diji and Amri. On the Indian side Kalibangan, Lothal and Rangpur were excavated during this period. Much new and extraordinarily important material has come to light in the course of these excavations and it deserves consideration as well. Several papers in this volume deal with Kalibangan and Lothal. For coverage of Kot Diji and Amri the comprehensive site reports should be consulted (F.A. Khan 1965 and Casal 1964a).

As new and important as this material seems to be, there is still a challenge for those who study this civilization. There is, for example, little in the way of knowledge concerning the specific institutional configurations of the Indus Civilization. Based on the presence of granaries at both Mohenjo daro and Harappa, Sir Mortimer Wheeler (1966:18) has suggested a redistributive economy on the Mesopotamian model. This inference has been challenged by Marcia Fentress (1976). But it has not been replaced by an alternative which provides an insight into the nature of Harappan economic organization. It is evident to many who have evaluated the quality of the data from the major excavations which opened this civilization to the world (Marshall 1931; Mackay 1938b; and Vats 1940) that they offer very little guidance in clarifying this problem. This suggests that renewed excavation is called for, with more clearly defined objectives incorporating elements of hypothesis testing. The apparent absence of

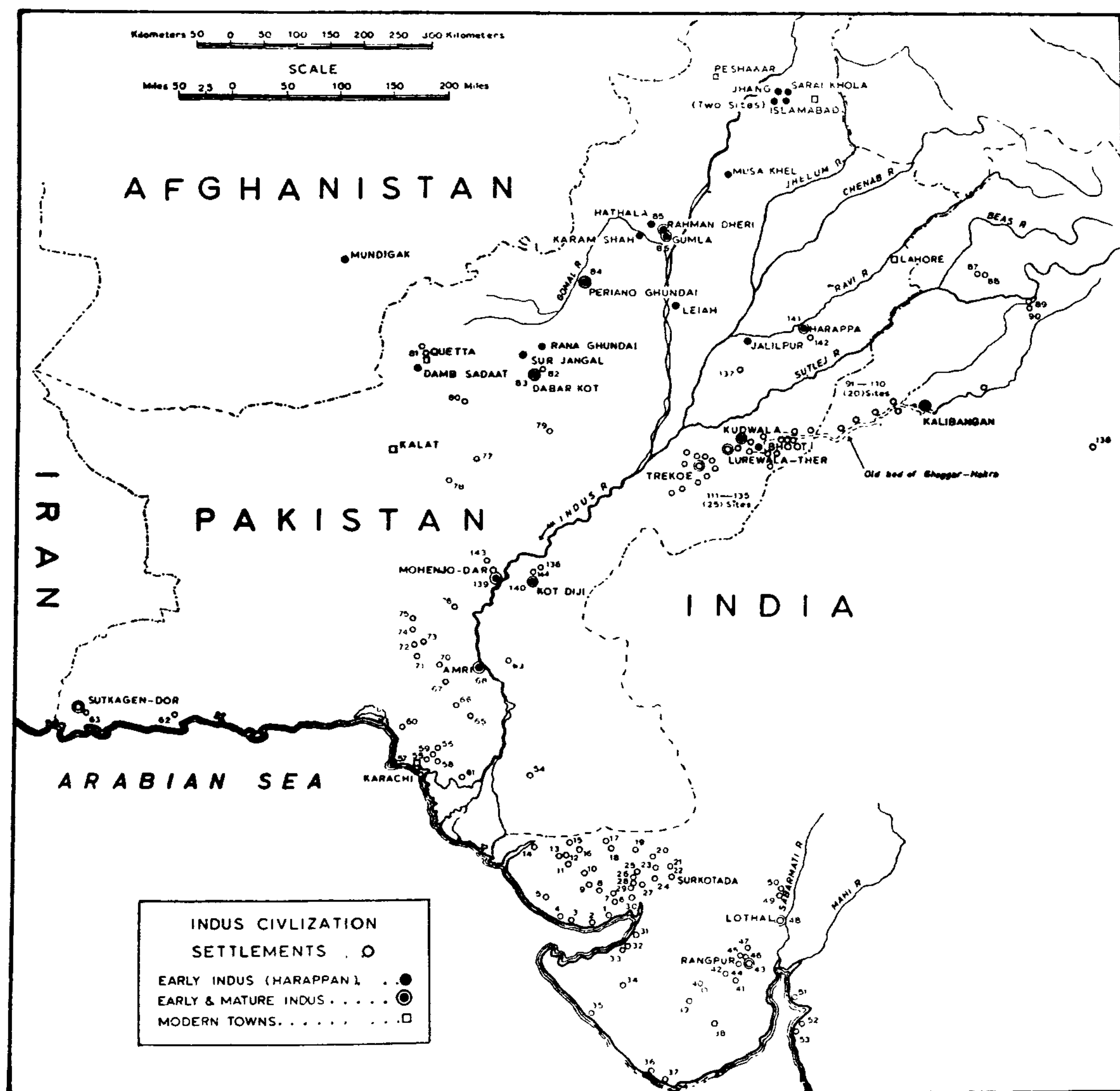


FIGURE A. *Settlements of the Indus Valley Civilization (1973)*
(Refer to Fig. 7.1 also)

1. Mundra 2. Navinal 3 Madeva 4 Todio 5. Naliya 6. Anjar, 7. Kotada 8. Buj 9. Kotada Bhadli, I, II & III 10. Nakhtarana 11. Desalpur 12. Narapa 13. Vada (Vigodi) 14. Lakhapat 15. Luna 16. Banni 17 Kotara 18. Nenu-ni-Dhar 19 Kotadi 20. Moruo 21. Kerasi 22. Surkotada 23. Selari 24. Rapar 25. Pabunath 26 Lakhapar 27. Kanth Kot 28 Khari-ka-Dhanda 29. Pirwada Khetar 30. Jhangar (Kutch) 31. Phala 32. Lakhabawal 33. Amra 34. Gop 35. Kindnarkhera 36. Somnath 37. Kanjetar 38. Veniavadar 39. Rojdi 40. Adkot 41. Bhimpatal 42. Babarkot 43. Rangpur 44. Devaliyo 45. Chachana 46 Goni 47. Pansina 48. Lothal 49. Koth 50. Nanasutaria 51. Mehgam 52. Telod 53. Bhagatrav 54. Gharo Bhiro 56. Amilano 57. Pir Shah Jurio 58. Nel Bazar or Allahdino 59. Goth Hassan Ali 60. Bala Kot 61 Gujo 62. Sotka Koh 63. Dasht-I 64. Sutkagen-dor 65. Shahjo 66. Karchat 67. Dhal 68. Amri 69. Chanhu-daro 70. Damb Buthi 71. Gorandi 72. Ghazi Shah 73. Lohri 74. Ali Murad 75 Pandi Wahi 76. Lohumjo-daro 77. Judeirjo-daro 78. Pathani Damb 79. Gand Damb 80. Kirta 81. Quetta Miri 82. Kaonri 83. Dabar Kot 84. Periano Ghundai 85. Rahman Dheri 86. Gumla 87. Katpalon 88. Nagar 89. Rupar 90. Bara 91–110. 20 or more sites in Bikaner (India) in the Sutlej, Sarasvati-Drishadvati valleys 111-135. 25 or more sites in Bahawalpur along dried bed of Ghaggar-Hakra 136. Kotasur 137. Vainiwal 138. Alamgirpur 139. Moenjodaro 140. Kot Diji 141. Harappa 142. Chak Purbane Syal 143. Jhukar 144. Naru Waro Daro

(The above list does not include many unpublished sites in Bahawalpur, East Punjab, Gujarat and in Sibi district, Baluchistan.)

Reproduced from Mughal (1973), *The Present State of Research on the Indus Valley Civilization.*

Harappan economic documents will certainly make penetrating insights into this institution a difficult one. But structures which functioned as granaries, workshops and warehouses can be projected as archaeological features which will emerge through appropriately sensitized excavation and analysis. It is also possible for renewed excavation to more comprehensively recover and record not only new and potentially important kinds of data, especially organic materials, but to handle the traditional range of artifactual data and their associations more rigorously. It is reasonable to expect that such procedures on newly excavated sites would yield interesting results concerning economic organization and processes.

The association between settlement and natural resources is also an area which needs more intensive investigation. Work on Indus settlement patterns in Gujarat (Possehl 1976) has indicated that the site of Lothal was a frontier settlement which played a key role in the Harappan economy at an interregional level. Given the fact that there are other outlying sites, similar to Lothal, distantly removed from the great urban centers, there is a possibility that similar implications for economic processes are to be gleaned from an investigation of these places as well. More specifically, settlements, such as Sutkagen dor on the Iranian border, Rupar to the north near the foothills of the Himalayas, and Dabar Kot in Baluchistan could be noted as potential places to begin such a study.

Settlement archaeology also has much to offer toward solutions of other problems. There are, for example, still many lacunae in our understanding of the Indus subsistence system. Even the question as to whether or not the Harappan inhabitants of Sind and the Punjab used large-scale canal irrigation has yet to be conclusively answered. Assuming the success of settlement archaeology in solving this problem in Mesopotamia (Jacobson and Adams 1958; Adams 1965; Adams and Nissen 1972) systematic exploration should be conducted in Pakistan, and India also, with this as a research objective. Settlement archaeology is probably the soundest approach to insights concerning the almost totally undefined Indus state. While it is true that settlement patterns cannot be reasonably expected to yield direct insights into the institutional heart of the political system, there are some dividends which can be anticipated. A much closer control over the settlement hierarchy, only fragmentarily perceived today, would seem to be a positive step in this direction. There is a range of analytical techniques derived from locational geography which could be used here as well. These could be employed to suggest both significant aspects of settlement function, and potential internal borders as well.

The full explication of the Harappan political institution will undoubtedly be an extraordinarily difficult problem in the absence of substantial written documents. It may be recalled that by far the largest corpus of Indus writing is engraved on square steatite stamp seals. The message length of these inscriptions is, on the average, between five and six signs. The general character of these seals leads us to assume that they were closely tied, probably in some complex way, to the identity of their original owner. Distributional analysis of these objects within Mohenjo daro, the only site with a sufficient body of adequately documented examples, has not produced coherent patterns (Fentress 1976). But a sign association study by Walter Fairservis (1976:110-111) suggests that the message content may be a system of rank and title. This is still a preliminary conclusion, and is now no more than a promising direction for future research; however, the discovery of an Indus system of rank and title would have obvious implications for both political and social organization.

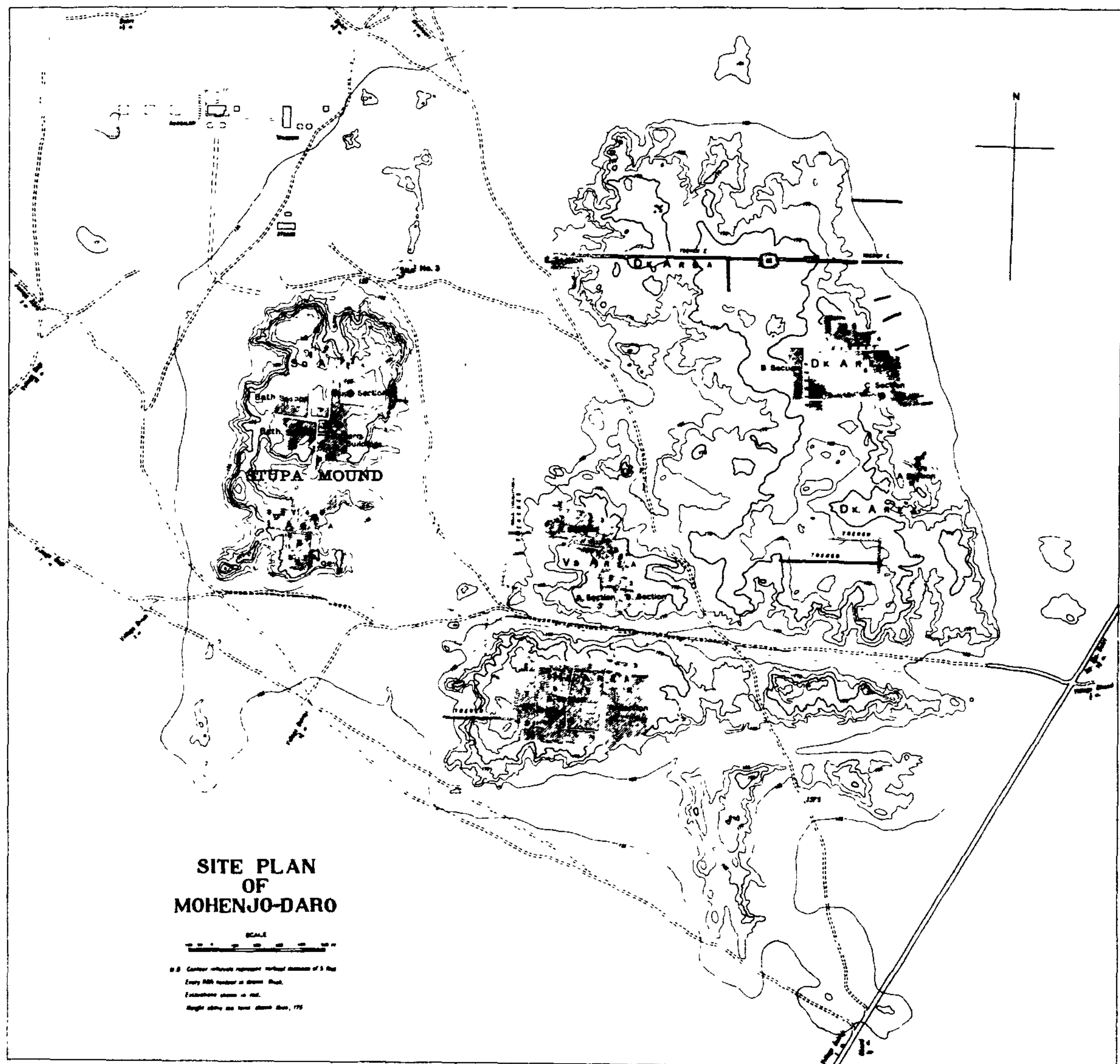
With the exception of obsolete concepts such as "priest-kings" there is little to be said of the structure of Harappan society. The inference from the seals is undoubtedly the most promising approach to this problem to emerge from recent research. Of course, burial data can and have been used to make inferences concerning pre-historic social systems. The application of such an approach to Harappan materials is discussed in the Introduction to Part I.

In addition to those directions for new research which seek essentially to describe, or characterize the structure and internal dynamics of South Asia's first civilization, there are a number of issues of a more purely culture-historical nature which ought to be noted in this regard as well. For example, the genesis of urbanism on the Indus plains has yet to be addressed seriously in anything other than one or two studies. In the best of these, M.R. Mughal admirably systematized the comparative stratigraphy of what he has called the Greater Indus Valley and Northern Baluchistan (Mughal 1970). During the period just preceding the development of the Indus

cities he isolated a ceramic complex associated with a few small finds or antiquities which forms a recurring body of material within his region. This consistent element is, however, associated with several much larger archaeological complexes which are quite identifiable and distinct one from the other. In other words, Mughal's Early Harappan complex "floats" as an island of recurring traits within a region of cultural diversity. It does, nonetheless, lend some sense of unity to the time just preceding the emergence of the Indus cities. Mughal's study is also useful in noting several features of material culture, settlement patterns and subsistence practice, which are a thread of continuity from the Pre-urban to the Mature or Urban configuration of the civilization.

It is becoming increasingly clear that the Early Harappan is a period within which there are few signs of germinating urbanism. For example, the Indus script has no genuine antecedent there, in spite of excavation at several sites (e.g. Kot Diji, Amri, Kalibangan, Bala Kot, Jalilpur and Sarai Khola). The Indus writing system *seems* to arrive within the Harappan cultural tradition as a fully

FIGURE B
Reproduced from Marshall (1938), *Mohenjodaro and the Indus Civilization.*



developed feature. Yet, surely there must have been a developmental process involved with something as complex as writing.

That this has not yet been found suggests that the period of transition within which the presumed development took place was very short, so brief that controlled excavation has yet to detect it. It should be kept in mind that the Harappan chronology is still based on radiocarbon, with a few check points in the Mesopotamian sequence. It then follows that the margin for error in the isolation of a chronological boundary such as the Early/Mature Harappan must be not less than one hundred years and is probably closer to two hundred. Thus we are dealing with a poorly controlled context within which such short-term change could be easily masked or "lost."

There are a number of other features in the Urban Phase of the Indus Civilization for which there are no antecedents in the archaeological record. The system of weights and measures, the style of many of the metal objects, the ceramic corpus if taken as a whole, architectural features such as the use of baked brick, drains, and the diversity of building form, town planning and the distinctive, but normative, organization of settlements with a high mound to the west and a lower habitation mound to the east, the development of a settlement hierarchy—all come to mind as striking features in this regard. Thus, in the Indus case it seems that there was a brief, critical period during the urbanization process within which these and other features developed.

This short period of intense change places the Indus Civilization in some contrast to Mesopotamia. In the latter case there is a much clearer long-term developmental sequence for at least the direct archaeological traits attendant upon urbanization. A gradualist evolutionary model seems to be a satisfactory perspective there. It does not seem to be as satisfactory for ancient India.

There are other processually oriented questions which still escape adequate exposition within the Harappan Civilization. The ultimate abandonment of Mohenjo daro, Harappa and a number of other settlements is one such question which will be discussed in a moment. But first a word on the Indus "monolith." The earliest investigators of the Indus Civilization, from Sir John Marshall to Sir Mortimer Wheeler, often remarked upon the seeming homogeneity of these remains. Harappan material culture was seen as virtually unchanging over five hundred years. So, too, on the spatial dimension did they note a homogeneity, in spite of the distances between sites. Mohenjo daro and Harappa are approximately 370 miles from one another. The large Harappan site of Sutkagen dor near the Arabian Sea on the Iranian border is 680 miles from Lothal, just north of the Gulf of Cambay in Gujarat. From the latter site to Rupar in the foothills of the Himalayas the straight line distance is approximately the same: 720 miles. There are even recently discovered Harappan sites in northern Afghanistan near Shortigai (Lyonnet 1977). The actual area encompassed by this civilization is difficult to compute since precise boundaries are not known, but even conservatively the size is three or four times the area of ancient Sumer.

This is a vast area, and five hundred years is hardly a trivial period. From my perspective as an anthropologist it seems unlikely that the monolithic, or changeless quality, of these remains is even an approximation of reality. There have been at least two extensive studies which have attempted to deal with this problem (Possehl 1974 and Fentress 1976). Their success indicates that similar work on different cultural dimensions would be equally productive in defining variability within both the spatial temporal dimensions of the Harappan civilization.

The so-called "end" of the Indus Civilization has been discussed at some length in the Introduction to Part IX. But since the focus here is more clearly programmatic a few additional words on the topic may not be redundant. The first point to be emphasized is that the problem seems not to be best stated as the "end" of a civilization; at least in the sense of a tradition, since there are abundant signs of cultural continuity in Sind, Gujarat, the Punjab and adjacent areas of North India (Possehl 1977). But the cities, Mohenjo daro and Harappa, were abandoned along with other places (e.g. Kalibangan, and Judeirjo daro) which seem to have been towns. Many smaller sites have discontinuous occupation as well. This phenomenon is however not applicable to all Harappan settlements For example, Chanhu daro and Rangpur have very clearly defined Post-urban occupations. Moreover, the process of abandonment, at least at Mohenjo daro and Harappa, took a considerable period of time. This is in the range of one or two hundred years if the "squatter levels" in the uppermost reaches of these sites can be used as a chronological guide. The pattern of

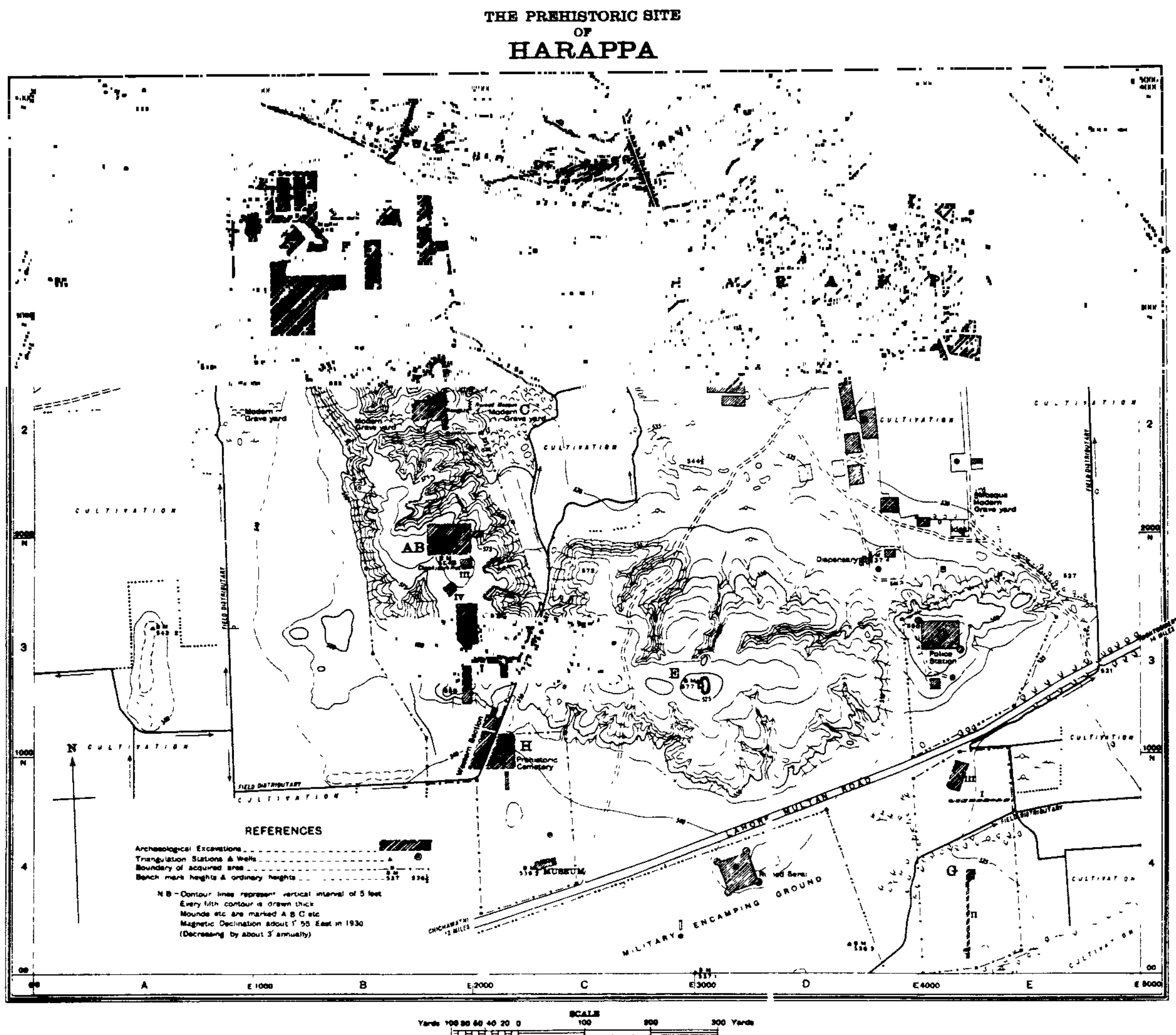


FIGURE C
Reproduced from Vats (1940), *Excavations at Harappa.*

change, then, seems not to indicate an end to this cultural tradition as much as a progressive modification of the distinctly urban form, a shift in the level of sociocultural integration.

There is in addition to the shifts in settlement occupation an interesting set of typological discontinuities associated with the Post-urban Phase. The most striking of these is the system of writing which was drastically altered and preserved only as occasional graffiti on pottery. The square steatite seals are confined to the Urban Harappan. So too is the standardized system of weights and measures. The typological list can be expanded to include the pattern of worked stone, triangular terracotta cakes, metal objects, bead forms and the like. This is a genuine "mixed bag" of cultural items, some of which seem to bear more directly than others on the problem at hand.

The process which eventually led to the gradual abandonment of the two cities and the other settlements, and which seems linked in some way to the typological shifts just noted, is best assumed to have been complex. Yet, what is it we are really attempting to explain? The previous sentence is, I think, a fair summary, and a telling one as well. It is a vague generality without significant detail, or historical richness which places constraints on hypothesis construction. The logic for programmatic purposes

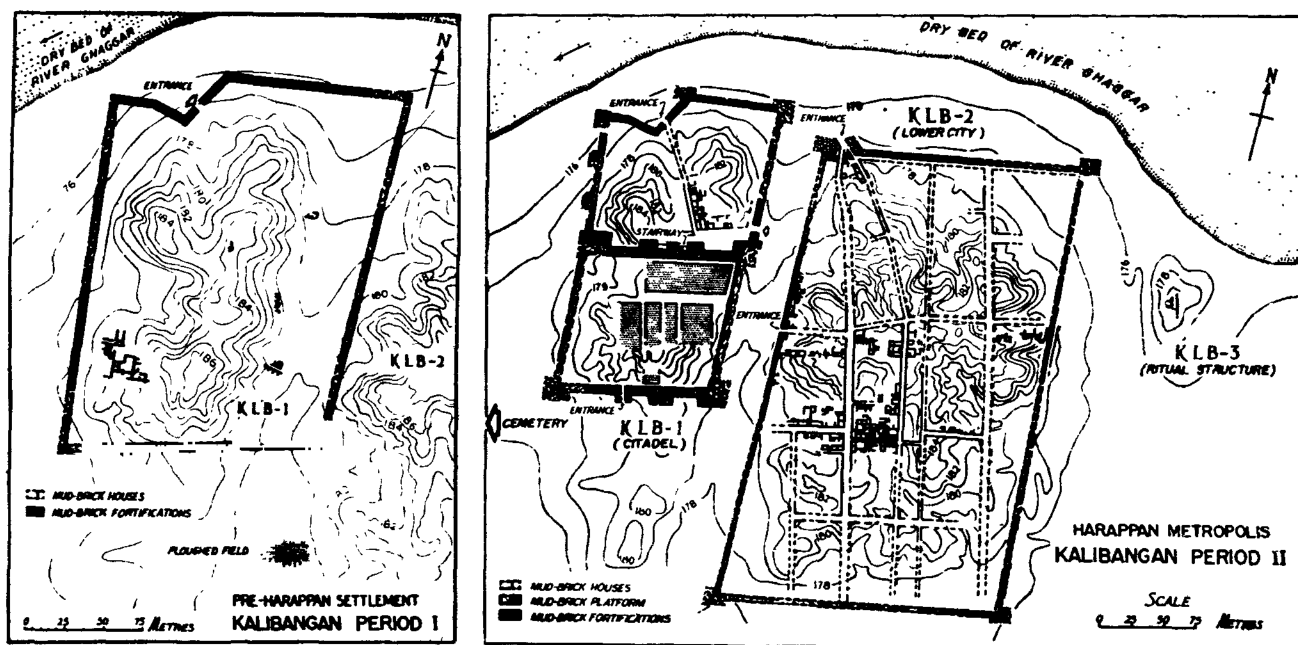
is then clear. Since it is essential to know what is to be explained before attempting to explain it, the priority must go to culture history. A range of important lucunae in our understanding of Harappan life ways, or institutional configurations, were noted in earlier paragraphs. Studies such as these, once complete, ought to yield interesting and important insights into processual issues relating to the Post-urban Harappan. So too might similar work on the Post-urban Phase itself be productive. Studies such as these are essential first steps in reconstructing the aspects of Harappan culture history which have a direct bearing on what we perceive today as the abandonment of Mohenjo daro and Harappa.

At the expense of seeming to be redundant let me restate the case slightly. The process which eventually led to the development of the Post-urban Phase of the Indus Tradition is not one with constraints on data which are sufficiently robust for us to confidently define the problem. The task for archaeology is then to do the kind of research which will ultimately correct this situation. Character studies, essentially the investigation of life ways, seem to be a direct route to this goal, especially if the Urban and Post-urban Phases can be contrasted in these respects so that we will develop a more encompassing sense of just what did change in the culture history of the Indus Civilization.

The foregoing is certainly not intended to be a comprehensive summary of every pressing, or worthwhile direction for research on the Indus Civilization. Nor should they be considered narrowly defined statements for action by those who are actually involved in work on things Harappan. They are instead intended to act as a guide for those who will use this book. It is hoped that they will place the papers which follow in some kind of historical perspective where the results of past scholarship can be seen as contributing to the evolving configuration of this field of study.

The book has been organized into eleven parts, each covering a separate aspect of the Indus Civilization. Some topics which might have been added, the genesis of urbanism for example, could not be highlighted in this way due to an insufficient body of appropriate material. In other subject areas it was impossible to include everything of value. This meant that at times considerable selectivity had to be exercised. In these situations newer papers were given a priority over older ones, and a concise statement, especially if it covered several facets of the civilization, was given preference over more lengthy

FIGURE D
Reproduced from *Expedition* (1975)



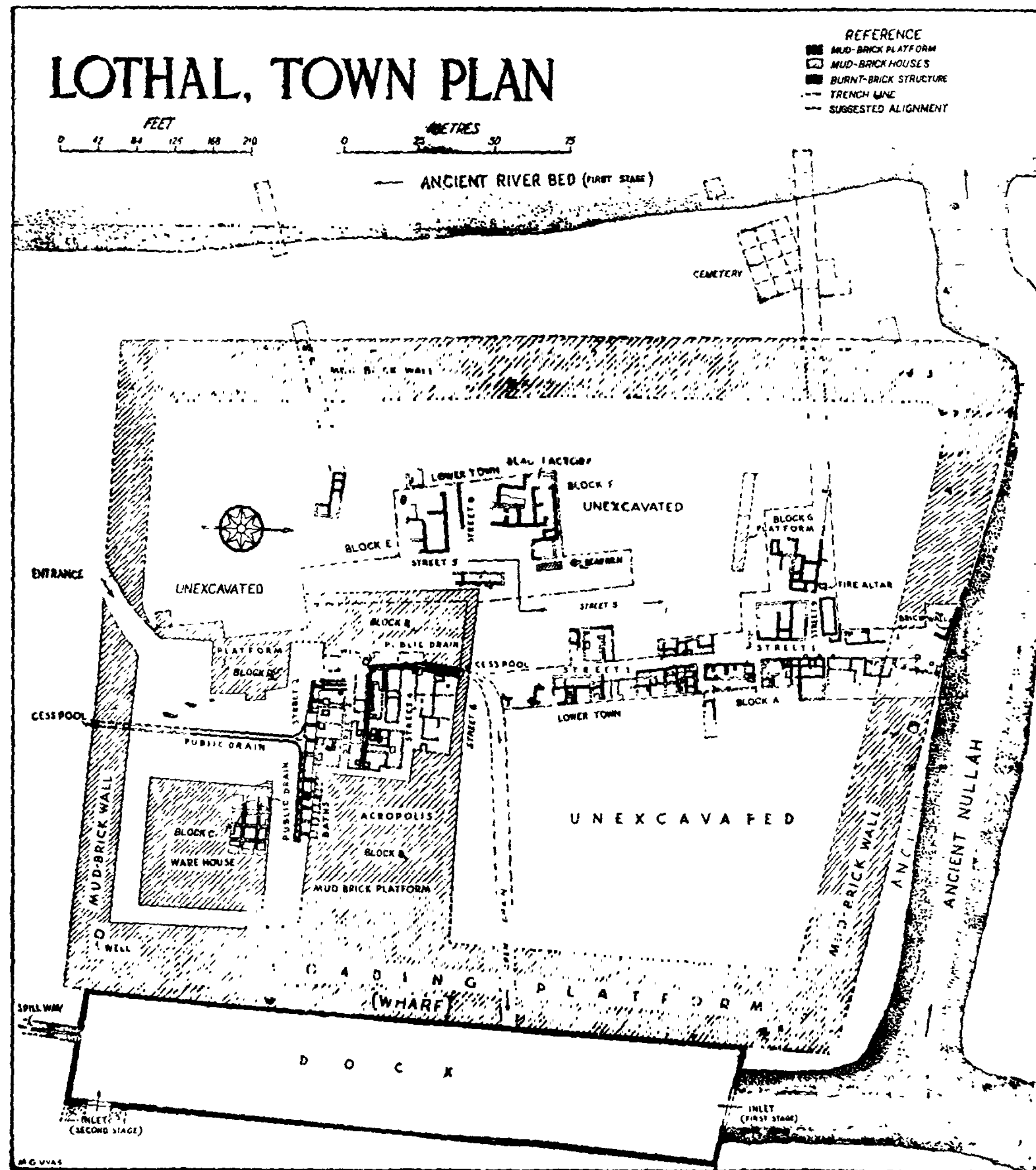


FIGURE E
Reproduced from Rao (1973), *Lothal and the Indus Civilization*.

treatments. If this selectivity has perhaps caused something important to be omitted, or the thought of a prominent spokesman to be slighted, I can only offer an apology and note that every effort was made to achieve representative coverage within a context where comprehensiveness was impossible.

There is a degree of arbitrariness in the composition of individual parts. This is due to the fact that the

best papers in any discipline probably have more than a single important aspect and it is therefore impossible to satisfactorily place them within an essentially static structure such as a table of contents. The composition of parts is thus largely a reflection of my view of this civilization.

Each part has been prefaced by a short introductory essay. These are intended to highlight the contents, place the material within some larger context and offer additional bibliography which will be useful in pursuing individual problems at greater length.

The preparation of this book required certain decisions concerning content and format. The major alteration of the original material has been the adoption of a single system of citation.[1] A good deal of bibliographic research was required to complete many of the older style footnotes; however, the system used here should enhance the readability of the text and allowed the book to be organized around a single bibliography. At times the text of individual papers was slightly altered. For example, the specialized transliterations were omitted from Oppenheim (1954) wherever possible. In addition, many illustrations, especially photographs, were deleted as an essential cost-saving device. The reader will note, however, that the book is still rich in illustrative materials.

This is not, in fact, my book. It is the product of the individual scholars whose original research has been compiled here. It is to them, and to their publishers, who generously gave the rights to reprint the papers, that the credit must go. Others who played a role in the preparation of this manuscript are: Jennifer Quick, Margaret Pugh, Patricia Canfield and Lisa Quigley. I would like to thank all of them.

GREGORY L. POSSEHL

[1]There were a few points in the editing process where it was impossible to do this. At times incomplete citations in the original did not allow a full bibliographic entry to be made. In other instances, the bibliographic resources available to the editor were insufficient for the original citation to be placed in the proper format. In instances such as these, a footnote has been used. C.J. Gadd's "Seals of Ancient Indian Style Found at Ur," because of its genuine charm and historical place in this literature, was left as originally printed.

Part I Civilization

Editor's Introduction



This part contains four papers dealing with the concept of civilization in general. They have been selected from an immense body of writing on this topic and should not be considered representative of all views. For example, the great historians, Spengler and Toynbee, are not included. D.D. Kosambi, who has much to offer with reference to Indian Civilization, has been similarly treated. The writings of anthropologists such as A.L. Kroeber, Milton Singer, David Mandelbaum, Clyde Kluckhohn, Margaret Mead and Ruth Benedict, all of whom are noted in the first paper by Robert Redfield, have also been omitted. This was necessary for reasons of space, and the fact that the focus of this book is the Indus Civilization, a member of a class, and not that class itself.

Robert Redfield's writing on the topic "civilization" is especially stimulating. In "Civilizations as Things Thought About" the reader is led along a path of contrasts appropriate to the notion of civilization. The paper thus introduces a breadth of concept which might be applied to the Indus Civilization. For example, he discusses the differences between "a civilization" and "civilization" more generally. He contrasts often used concepts such as "form and style," "structure and pattern," "society and culture" relating to civilizations as "formed things." These are, in a historical sense, important issues which must be controlled in understanding much of the writing on the Indus Civilization. If the reader is taken by Redfield's approach further study ought to include his *The Folk Cultures of Yucutan* (1941), *The Primitive World and its Transformation* (1953) and his collected papers edited by Margaret Park Redfield, *Human Nature and the Study of Society* (1962).

The concept of cultural ecology which places man within a dynamic set of relationships linking human systems to the natural world has a long and distinguished history in anthropological and archaeological research. In "The Natural History of Urbanism" Robert McC. Adams takes this perspective on the growth of early civilizations. Of particular interest is his discussion of the relationship between the city and its hinterland and factors which can be seen as causative in the development of urban societies. He suggests that there is a complex set of inter-related factors which ought to be investigated by those interested in the origins of the state. Some of these are more purely sociocultural (e.g. military advantages conferred on those types of social organization best able to equip and mobilize large bodies of militia or professional soldiery), and some are environmental (e.g. the existence of juxtaposed and interdependent subsistence zones which fostered the formation of symbiotic regions). Those interested in the early phases of the Indus Civilization will find this paper useful if they are searching for a route out of the striking empiricism which characterizes so much of Indus archaeology.

"The Cultural Evolution of Civilizations" by Kent V. Flannery was selected because of its eclectic nature, and the fact that it argues for a general systems approach to understanding civilizations or the state, wherever they might be found. The systems paradigm has yet to be used in South Asian archaeological work and this introduction, it is hoped, will stimulate others to adapt it to an understanding of the Indus Civilization. Clearly Flannery has offered an organizing principle which goes well beyond description and comparative stratigraphy in understanding the growth of urbanism.

The fourth paper in this part is V. Gordon Childe's "The Urban Revolution." Childe's ten criteria for

civilization have been widely cited and retain an assured place in the history of archaeology. Today, however, they have been largely overshadowed by far more penetrating insights into the nature of urbanization. In fact, his criteria amount to a straightforward typology of traits not only variable in significance but clearly inter-related. The list of ten can be easily reduced by half without significant loss. But those that remain (the state, social stratification, and craft specialization, for example) bear a striking resemblance to what Robert Adams has called the "core trends" of urbanism (1966:16). This suggests that there is still a utility at the level of description, not necessarily analysis, to what Childe has offered.

Adopting for the moment a characterization of the Indus Civilization suggested by the residue of Childe's typology an interesting question arises. To what extent can we legitimately infer central factors such as social stratification and class, the state, kingship and taxation from direct evidence? For those who know the data I think there would be agreement that it cannot be confidently done, and one might then be led to question the basis on which the term "civilization" has been applied. Of course the superficial answer to this is not difficult to find: There are two cities and there must therefore be civilization. But this seems to be confusing dependent and independent variables. Surely cities are not just large settlements but are best defined by the distinctive kinds of activities which go on within them, and these are in turn determined by factors relating to Adams' "core trends." Thus cities, where they are present, are symptomatic of deeper "core" processes and without a clear control over these it may be best not to employ the term "civilization" prematurely.

Using recently developed methods it is possible to recover a great deal of information relating directly to both social and political organization. Take, for example, the sophisticated studies of burials which seek to discover co-variation between the quantity and quality of burial goods and the demographic features of the interred individuals (Rathje 1970). The employment of various techniques of locational analysis has proved useful in suggesting boundary areas within settlement patterns (Adams and Nissen 1972). These approaches and others point in a clear direction to future research on the Harappan Civilization in that they provide concrete strategies for testing key assumptions concerning the nature of this civilization or whether it is a civilization at all.

Civilizations as Things Thought About

ROBERT REDFIELD

The comparison of civilizations requires, or at least leads to, a conception of civilizations as members of a class. As a necessary part of such thinking, it leads to thinking of any one civilization as having a formed and defined identity, that same kind of formed and defined identity that other civilizations have.

The following pages express an attempt to develop these propositions. They do not bring the reader to definitions of civilizations. It will not even be asked here if French or American life is to be recognized as a civilization or how either of these entities is to be distinguished from or related to that plainly greater entity that we call Western civilization. Such problems as arise in connection with such other entities as have relatively mixed historic origins, as Latin America, are not to be considered: these may or may not be recognized as civilizations according to the requirements of definition that develop in connection with particular questions about civilizations, none of which is reached in the present discussion. The matters now in mind are preliminary to recognitions and definitions of particular civilizations. What is to be offered is a set of propositions as to the nature of those definitions which, in studying civilizations comparatively, it will be necessary to make.

Another limitation upon this effort is to be announced. Primarily in mind are those civilizations that are obviously local and very ancient and yet present now. The attention is upon that arrangement of human life and institutions that has been with us for a long time. Chinese, Indian, Western, Islamic—at least these—civilizations have been and still are recognizable to common sense; they are divisions or separations of long duration and considerable distinctness within humanity. If, when we speak of "world civilization," we mean either an extension of one of these four or more so as to take the place of others, or the development, by a mixing and homogenization, of a kind of living becoming uniform everywhere, such a "civilization," if civilization it be or would be, is not here in view. The distinguishable civilizations with early historic beginnings and persisting identity and influence are still with us, and it is about these that the mind turns in these pages.

In developing the idea of thinking about a civilization as a formed thing, it will be helpful to distinguish that mental or investigative activity from others also having to do with civilizations.

We may distinguish getting information about a civilization, or about a civilized people. If we receive information as to the rate of growth of the population of India and the rates of growth of its agricultural and industrial production, we have information about India. This will help us to predict the course of some kinds of events in India, or help us to form judgments as to action or policy. Such acquisition of information may occur entirely without the thinking of India as a formed thing of the mind, a member of a class, civilizations.

We may distinguish, also, becoming acquainted with a civilization within one's self. Every member of a civilized society of course becomes acquainted with his civilization, however imperfectly, in the usual course of his life within it. One outside of that civilization may also, though with more required effort, I suppose, become acquainted with that other civilization. He may do so by living within it and there meeting the people and experiences which express that civilization;

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and he may do so, in ways obviously more limited, from afar, through such expressions of that civilization as may be there communicated to him. "Expressions" is here a significant word: it points to those utterances of representatives of the civilization which best communicate to an outsider "how it feels" to be one of that other civilization. We speak of "expressive documents"—autobiographies, songs, stories. And we recognize the expressive character of the arts for one seeking to get acquainted with a civilization. The "formal document," the statistic, or the considered and objectified proposition, is less communicative of that experience which is "inner" to the civilization, which makes the substance of the lives of its peoples. In coming to think about a civilization as a formed thing, one may attend to these expressions of the life within in the effort to relate the substance of that civilization to its form or structure. But the thinking of the civilization is a matter of form or structure, a making in the mind of an abstraction, a construct, a mental device usable also in reference to other and compared civilizations.

We may yet further distinguish (while later making a relation therewith to the immediate subject) thinking about a civilization from the thinking about or the study of something less than, but within and of, a civilization. Every scholar or scientist concerned with something about or from or of a civilized people addresses himself to something of this lesser but connected kind. The student of the Confucian classics or of Islamic law or of business cycles in the United States or of the Japanese novel or urbanization in Sweden is concerned with something less than but of a civilization. To carry forward his thought about any of these things he need not make the effort to conceive of his civilization as a construct, a formed generic idea, a structure of ideas relevant, not only to the one civilization, but to others.

Anthropologists have been in very large part concerned with peoples not civilized. But in recent times they have shown a rapidly growing disposition to study civilized peoples, especially particular civilized communities. Relevance of some of these recent studies to the comparative study of civilizations as formed things of the mind has been reviewed, although not with regard to that relevance, by David Mandelbaum (Mandelbaum 1956). This review shows, indeed, that with one important exception these studies do not define civilizations as "constructs," nor identify the class of things, civilizations. Most of the publications that Mandelbaum reviews are accounts of villages or towns or cities within civilizations; included are Warner's publications on American communities and institutions; the largest number of works reviewed are studies of peasant or rural communities in Europe, Asia, or America, North or South. Included in his review also are studies made by anthropologists of modern nations, either at first-hand or "from a distance": Lowie's book about Germany; the study of the European Jewish *shtetl, Life Is with People*; the many studies of national life or national character made by Benedict, Mead, Mead and Metraux, and others.

These are all studies of something less than but within or connected with a civilization. They are not expressly related to a generic concept of civilization such as Toynbee has in mind in his work. There is perhaps an approach to a representation of an entire civilized way of life in the study of the *shtetl*, a component of Western civilization, and Benedict's book, *The Chrysanthemum and the Sword*, whatever its limitations, may be seen as a portrait of Japanese civilization. But neither of these two studies defines the construct, civilization. They represent excursions of anthropology into the group-phenomena of communities of peoples derived from and within and of civilizations. They do raise questions, new to anthropology, as to what are the limits of the entity studied and as to how these more complex and less studied entities—as compared with tribal communities or primitive bands—may be studied and conceived.

Among the works reviewed by Mandelbaum there is, however, one full exception to these statements. Kroeber's work does involve a conception of a civilization as a formed thing and a recognition of civilizations as a class of phenomena. Kroeber characterizes civilization, any civilization, principally as a congeries of styles. He shows us regularities in temporal patterns of styles; he considers the combinations of content, style, and value in civilizations; he examines the developmental flow of cultural products. He is conceiving civilizations as having form, with content expressed in styles, through time. A construct *is* being developed. His work does not derive from the study of particular local communities. The "matter" of his civilizations is cultural substance and form, and its outlines extend through much historic time.

The word "civilization" will be here used for a subclass: a more particular kind of culture or society. This is, I think, Toynbee's use: "Primitive societies

and civilizations . . . correspond to a real specific difference within the genus 'societies'" (Toynbee 1945/61:I, 147-8). Concerned with both surviving and extinct civilizations, he recognizes less than thirty; the number varies throughout his work: there are mentions of twenty-two, twenty-three, twenty-six and of "something less than thirty." But the formal nature of the generic thing, civilizations, is not fully defined. He does not, for example, make any intensive examination of the differences and resemblances between Incaic and Egyptian civilizations. He is, however, much concerned with problems of delineation of civilizations; he does explore margins in time and space of some of them. He is concerned with "where you stop" in pushing outward or backward to find the limits of a civilization. And of course his very great interest lies in the genesis and filiation of civilizations.

Toynbee perhaps somewhat overemphasizes the separability of the subclass, civilizations, from primitive societies. The two categories appear in his pages in a contrast not often qualified by intermediate cases. He does not give much attention to societies that might be intermediately marginal, for instance, West African states. His terms "arrested civilizations" and "abortive civilizations" point to but do not define societies that are not or are not quite civilizations; and his interesting discussions of barbarians, as people in certain kinds of relations to civilized peoples, also suggest but do not fix a class or classes distinguishable from his twenty-odd civilizations. The definition and understanding of barbarians as a kind of society or culture or way of life remains unfinished business for us all.

Kroeber's thought as to distinguishing civilizations as a subclass within the larger class, societies and cultures, is in substantial agreement with the proposition which began this chapter: "Some cultures and societies are, more particularly, civilizations." There is a difference from Toynbee, as I see it, only in the emphasis he gives to the word "culture" and to its use for high or advanced cultures as well as low or not-advanced cultures; and in his readiness to allow the two words to be used, except in special contexts, interchangeably. Kroeber writes, "By many anthropologists, ever since Tylor, the words 'civilization' and 'culture' are often used to denote the same thing; and always they denote only degrees of the same thing" (Kroeber 1948). He says much the same in the recent lectures on style and civilization: "Like many anthropologists, I use the word civilization almost synonymously with the word culture. At any rate I try to put no weight on the distinction. There is a widespread usage of the term civilization as meaning advanced or literate or mainly urban culture. With this usage I do not quarrel, but I have tried to choose between the two near synonyms in such a way that the reader would realize in any given situation whether I meant the more general or the more slanted sense" (Kroeber 1957).

But while preserving the two terms for equivalent applications, to Andamanese, I suppose, as to Western culture, Dr. Kroeber in fact, in the recent book, separates out the advanced cultures (my "civilizations") and shows us that he is thinking of them, and not of the primitive, not-advanced cultures. An emphasis *is* given. His topics refer almost exclusively to the civilizations: his discussion of styles in women's dress relates to Europe with references to Rome and to China; the chapter on the fine arts has examples from civilizations only—(and, by the way, do the primitive cultures produce "fine arts"?). The same may be said of the facts drawn upon in his discussion of the clustering of genius; and the philosophers of history whose work he analyzes are all concerned with civilizations, not primitive cultures—Spengler, Danilevsky, Toynbee, Sorokin. The book is about the advanced cultures, taken apart from the primitives, however carefully the two terms are preserved for both.

In these pages the attention is more explicitly upon the difference between the two things for which the near-synonyms stand. Taking "civilization" to mean a special kind of society and culture, what is to be the definition of this special kind?

As already said, no definition will be attempted here. I do, however, now indicate a preference for one of two possible ways of conceiving the difference between societies and cultures which are civilizations and those which are not.

One way, which I do not prefer, is to think of civilizations and non-civilizations as closed classes. In such a view a civilization is to be recognized in the presence of one or more diagnostic criteria, and the non-civilization in the absence of such a criterion or criteria. Gelb (1952) identifies civilization closely with the presence of writing so flexible as to express most of what people find themselves wanting to say; it follows for him that only a small number of Old World societies or cultures are or were civilizations; the possible civilizations of the New World, not having such writing, are not civilizations. Such a categorization meets difficulty when the variety of systems of notation is examined and one finds that, however

limited in its possibilities of notational expression were or were not the *quipus* of the Inca, the glyphs of the ancient Maya, and the early Chinese inscriptions on the divinitory bones, each could record something and did not, so far as we know, record everything. Moreover, and obviously, this mode of defining civilizations puts aside the many other respects in which the "great cultures" or "complex societies" came to differ from primitive societies. This way of sorting out cultures and societies puts together some things that are also importantly different and separates some things that are importantly alike.

Much the same could be said of definitions of civilization in terms of the one criterion, the presence of cities or of urbanization. There are or have been societies and cultures in which cities played small part and yet which in other respects we do not hesitate to call civilized rather than primitive. Coulborn is right in saying, "The term urban is quite wrong as applied to feudalism" (Coulborn 1956). And the other difficulty, mentioned in connection with writing, appears here also: What is a city? Uaxactun was a very different kind of thing from London.

As one adds to a single criterion others, so that a cluster of diagnostic traits takes the place of a single criterion, one attends to more of that which we find in such societies and cultures as common sense willingly calls civilized. But the difficulties of agreeing upon the definitions of the criteria, so as to produce drawn lines separating the civilized and the non-civilized, are multiplied. There is an interesting exchange between Toynbee and Robert Bierstedt (Toynbee 1945/61:IX, 188-9) about the attempt of the latter to define civilized life by four (or five) criteria: writing; having a history; isolation; smallness. Toynbee rejects all four. Furthermore, definitions of civilization may vary with the kind of information available about the civilizations which form the basis of the definition. The ten traits or elements by which V. Gordon Childe identifies arrived civilization in contrast to the antecedent non-civilized conditions (Childe 1950) are (with the exception of "naturalistic art") aspects of material development or of the political and social concentration of power. One sees that the choice of these ten derives from the nature of archeological evidence for ancient human life and also from Childe's intellectual predilection for material determinism. One feels that if ten criteria were sought by looking, not at the archeological information about ancient peoples, but at the world civilizations of the present day, a different list of ten might be chosen.

Instead of relying upon the presence or absence of certain criteria to make a society or culture either civilized or not civilized, I prefer a form of thought that sees a society to be civilized "to the extent and in the respects that" it has one or more of very many qualities we find attributable to Toynbee's civilizations or Gelb's or to those picked out by anyone interested in making the distinction. In such a way of thinking, civilizations are not a boxlike class but rather a multiform tendency of development on a complex continuum of societies and cultures.

I think that I suggested such a way of conceiving civilizations when I attempted in analogous terms to define "the folk society" (Redfield 1947 and 1953a). The folk society is that abstract kind of society that is not civilized; it is another end of the complex continuum. But, for a student of civilizations, that attempt was a beginning from the bottom end. And though then I referred to an opposite end as "urban," I think now it had better be called "civilized." Urban society has been characterized as a type; civilized society might be. Whatever content be put into the proposition that would characterize it as an end of a complex continuum, its form may be indicated by beginning such a proposition as: "To the extent that a society is large, has multiplied communications within and without among varied kinds of people, has divided labor, especially that addressed to intellectual and aesthetic production, has developed deliberate design in personal and collective life—policy, legislation, enacted institutions," and so forth. But this has not been done. Such a definition would recognize civilization as beginning variously, as one or another aspect of a loose complex of associated elements of civilizational living went forward ahead of, but not entirely independent of, some of the others—Maya astronomy; Inca public administration.

Conceiving civilizations so, one can arrange them in a rough order from least civilized to fully civilized; the order will vary as emphasis is placed on one or another characteristic (writing; urbanization; reflective and creative works of the mind; etc.). But the order will not be entirely reversed by altering these emphases: the societies that have cities are more likely to have writing, to produce reflective and creative works of the mind. The many characteristics of civilization then are like threads of new or growing color introduced into the loose web of societies and cultures which history weaves. Some threads begin in primitive life and

continue into civilization; but other threads are fully evident only to the degree and in the way that that society becomes civilized. And the webs of the distinguishable civilizations are different, yet similar in ways that we wish to explore.

Further, the distribution of actual cultures-societies along this continuum is not an evenly spaced distribution. There are very many known societies near the primitive end and only a few near the civilized end. Toynbee's assertion, "one is elephants and one is rabbits," has truth. History and the earth's surface have room for only a few civilizations. However we define civilization, we shall sufficiently agree that in dealing with China, India, the West, and some ancient great cultures, we are dealing with civilizations. The marginal or uncertain cases will attract our attention, in thinking about civilizations, in so far as and in the particulars that appear most clearly at the civilizational end of the loose web.

I pass over with brief mention only other meanings which the word civilization has served in the thought and writing of those who have other matters in mind than the study of civilizations comparatively, or who, with that same interest, have employed the word differently. From "civilization" as a subclass of societies or cultures we may distinguish at least these other uses of the word.

(1) For a highly valued way of life of cultivated and enlightened people. This meaning is developed in Brinckmann's article on "Civilization" in the *Encyclopaedia of the Social Sciences*.

(2) For the instrumental aspect of any culture, as contrasted with its more ultimate ends [as used by MacIver (1931) and others].

(3) For an inescapable stage in the life-history of all cultures. Spengler so uses it, with disparagement.

(4) For a class of event, perhaps one coalesced events-group as in the writings of V. Gordon Childe (1939b). This use approaches that employed in these pages.

A civilization may be conceived of as an object or entity. These two words are here equivalent to "thing." "Everything which can be thought of at all exists as some sort of entity," wrote William James. The term includes things remembered, mythical objects, things thought about by any thinker. Civilization is one such thing. I do not go into philosophical questions as to the nature of reality of the thing, a civilization. Such problems arise as well from consideration of primitive cultures and societies. In a civilization, less of that whole which makes up the thing is directly viewed; more is out of range of the observer's immediate sensory experience; the civilization, as thing, may be more difficult to conceive. But, like Andamanese society or Hopi culture, it too is a thing, object, entity.

I introduce four terms for developing a conception of a civilization as a thing that is formed in the mind, a "construct." No great importance is attached to the terms; others would do; and it may be that not all of these are needed.

Things may have identity and also delimitation, unity, and form. Identity is the separation of this thing from all else, vague or precise. Delimitation is marking the limits of this identity, where it is no longer that thing. This is easy in the case of a chair or a horse, harder in the case of cultures and civilizations. "All cultures have fuzzy edges," writes Kroeber. So do societies, although island Tikopia society is less fuzzy around the edges than is Western civilization. Toynbee is much concerned with delimitation, spatial and temporal. He reaches backward in time and also allows his mind to range outward from some approximate center of a civilization, as Rome, until he comes to a place where he stops. In discussing Western civilization, he asks: Were the Turks part of Western civilization at the time of the Renaissance? No, he concludes, because Italian ideas then influenced them very little while Islam influenced them much: an eastern limit of Western civilization at that time is thus discovered. The delimitation in time is accomplished by going backward in time until that civilization comes to be "presented to us in terms of something other than itself" (Toynbee 1945/61:I, 40). This point in time he fixes at approximately 775 A.D. when he finds himself dealing with the history of a society to which the Roman Empire belonged. Childe is concerned with definitions of a civilization at the temporal point of its beginnings: a culture has become civilized (is no longer pro-civilized) when it has Childe's ten criteria. Others recognize "town life" or "incipient civilization." These are problems of delimitation, of bounding the identity, of this civilization.

All matters of delimitation are inseparable from characterization—the recognition of the attributes of this civilization. The way in which a civilization is conceived, substantively, affects the delimitations reached. Toynbee emphasizes as substance universal religions and universal states: where and when these two occur, there is, and no farther, the civilization. Kroeber (1953) de-emphasizes language, religion,

political or military development, also economy and technology—these are substantial attributes of civilizations, but they do not coincide. He puts emphasis on "style." There results a delimitation without edges—it "floats" in time and space, but it lies in time and space, of course.

The *unity* of a civilization is, I suggest, its identity as defined by its substantial qualities. A civilization, like other cultures, may be thought of, and usually is thought of, not as a congeries of perfectly heterogeneous elements, but rather as having a oneness from attributes that refer to very much or even all of it. Just as Fortes says Tallensi society is this one social structure, or as Kluckhohn says that Navaho or village Mormon culture is conceivable as this set of values, so it may be possible to say that Chinese civilization is or was a characterizing set of kinds of social relationships or, perhaps, a persisting, prevailing set of views as to the nature of what is and ought to be. In making any such attribution, the thing, the civilization, is given not merely identity, but unity. In so far as one is aware of particulars in the thing, elements within it, the attribution of substantial qualities of this inclusive nature asserts unity, however imperfect, of the thing. Even that historian who is farthest from the scientist's disposition to abstract and generalize gives his kind of unity to a civilization by choosing from out of all that happened, or that he has any way of inferring to have happened, certain events, and not others, and by relating some of these events to others. Every history has some unity, and, if a civilization is its subject, there is unity seen in or perhaps given to that civilization. Of course the desire to compare civilizations—our subject—greatly affects and encourages the intellect to see or give this unity.

By *form* I mean that in the thing (here the civilization) that remains the same when everything else changes. The notion begins, then, with the popular meaning of "form" as figure or shape, sensible form, and moves on to include in the expanded meaning here employed, figures or shapes which are not sensible, conceived arrangements of parts that persist when the more particular contents of the parts are altered. Just as the form of a proposition may remain the same although its sense, its content of meaning, changes, so a culture or a civilization may be thought of as having a form that remains the same while the content, the institutions, usages, beliefs, change.

In anthropology, at least in dealing with primitive societies and cultures, the commonest words for forms are "pattern" and "structure." We are aware of a certain opposition between these two, as if the choice of the one made difficult the employment of the other term. Dr. Kroeber [as Lévy-Strauss (1953) reminded us] once put in writing his opinion that this word (structure) adds no meaning when it replaces others in describing either personalities or social structures. British students of societies, on the other hand, exhibit their discomfort with the word "pattern" and with terms associated with it: configuration, style. We are aware, too, I think, that this pair of terms and ideas as indicative if not always denotative of the form of a culture or society, primitive or civilized, derives from somewhat contrasting conceptions of traditional human groups, connected respectively with the words "culture" and "society." Also, we feel that each evokes its corresponding image: "structure," a machine or organism; "pattern" or "style" or "physiognomy," a work of art, a face, or perhaps a personality. But both are forms of cultures or societies. One calls to mind something impersonal, non-human or even mechanical—a balance of forces, perhaps, a system of exchanges. The other calls to mind one or another expression or representation of humanity, as in an aesthetic or intellectual work or in a countenance or character. I hope to avoid or postpone problems of choice between these two contrasting, possibly antithetical, conceptions of form in the thing that is a civilization by using, for the present, the word "form" for both kinds. Fortes, in describing the African Tallensi, Radcliffe-Brown in presenting to us an Australian band or tribe, Eggan (an American) in giving us the Western Pueblo peoples, conceive these wholes as societal structures, as arrangements of kinds of people (roles and statuses) in systematic relationships to one another. On the other hand, it is as configurations, systems of ideas, values, or choices of goals for life that we see the peoples described by Benedict in her *Patterns of Culture*, those described by Mead in her *Sex and Temperament in Three Primitive Societies*, the Sudanese Dogon reported by Griaule, or the Texas homesteaders recently described by Evon Vogt. In some works, as in *The People of Sierra* by Julian Pitt-Rivers, there is a kind of balance between societal structure and pattern.

This distinction between structure (societal) and pattern to the representation of civilizations or of some totality of civilized life is with more difficulty applicable. Such descriptions as we have from historians are not so obedient to conceptions of formed things of the

mind, of constructs. Burckhardt gives us an account of the Italian Renaissance which is, I think, predominantly a representation of values and view of the world, but in close association with institutions and individuals that embody these values and views of the world. The studies of "Chinese Thought" that I have seen also put forward a "pattern" of idea and value, but again with close interconnection with event and institution and without explicit definition of an inclosing transferable mental form for the whole. Toynbee is, I think, more inclined to conceive his civilizations as societal than as cultural, but it is the delimitation of each civilization, the life-course of each and all, and the historical or filiational relations of one to another that concern him, not the structure or system that a civilization may be conceived to be at any one moment of time. Civilizations are seen as a form of genesis, transformation, procreation, and perhaps disappearance.

These last words suggest another distinction with respect to characterizations of civilizations as members of their class: that kind of characterization that sees a civilization as an order of successive forms versus that characterization that sees a civilization now, or at that one period—in Radcliffe-Brown's terms, diachronic versus synchronic characterizations. In the study of primitive societies and cultures, where little is ordinarily known of the past, we are necessarily drawn to conceptions of synchronic form. Civilized peoples, as in the studies of national character, may also be so conceived. But "a civilization," as something not the same as a civilized people, seems strongly to connote its long history, about which a good deal is known, and it is not surprising that Toynbee and Kroeber become there interested in the series of developments of forms. Setting aside the common and etymologically rooted meaning of "cycle" as something that comes around on itself like a wheel, we may here use the word, as does Coulborn, for any succession in which forms run to their completion. Then Toynbee thinks of civilizations as cycles of transformation, procreation, and so on, and Kroeber thinks of them, principally, as cycles of or within styles. I think—but I am not sure—that Spengler is concerned with both cycles and synchronic configurations. I suppose that synchronic and diachronic conceptions might be variously combined in the conception of civilizations as constructs. A series of successive synchronic forms—the medieval configuration in Italy followed by that of the man of the Renaissance, for example—would be one such combination.

The Urban Revolution

V. GORDON CHILDE

The concept of "city" is notoriously hard to define. The aim of the present essay is to present the city historically—or rather prehistorically—as the resultant and symbol of a "revolution" that initiated a new economic stage in the evolution of society. The word "revolution" must not of course be taken as denoting a sudden violent catastrophe; it is here used for the culmination of a progressive change in the economic structure and social organization of communities that caused, or was accompanied by, a dramatic increase in the population affected—an increase that would appear as an obvious bend in the population graph were vital statistics available. Just such a bend is observable at the time of the Industrial Revolution in England. Though not demonstrable statistically, comparable changes of direction must have occurred at two earlier points in the demographic history of Britain and other regions. Though perhaps less sharp and less durable, these too should indicate equally revolutionary changes in economy . They may then be regarded likewise as marking transitions between stages in economic and social development.

Sociologists and ethnographers last century classified existing pre-industrial societies in a hierarchy of three evolutionary stages, denominated respectively "savagery," "barbarism" and "civilization." If they be defined by suitably selected criteria, the logical hierarchy of stages can be transformed into a temporal sequence of ages, proved archaeologically to follow one another in the same order wherever they occur. Savagery and barbarism are conveniently recognized and appropriately defined by the methods adopted for procuring food. Savages live exclusively on wild food obtained by collecting, hunting or fishing. Barbarians on the contrary at least supplement these natural resources by cultivating edible plants and—in the Old World north of the Tropics—also by breeding animals for food.

Throughout the Pleistocene Period—the Palaeolithic Age of archaeologists—all known human societies were savage in the foregoing sense, and a few savage tribes have survived in out of the way parts to the present day. In the archaeological record barbarism began less than ten thousand years ago with the Neolithic Age of archaeologists. It thus represents a later, as well as a higher stage, than savagery. Civilization cannot be defined in quite such simple terms. Etymologically the word is connected with "city," and sure enough life in cities begins with this stage. But "city" is itself ambiguous so archaeologists like to use "writing" as a criterion of civilization; it should be easily recognizable and proves to be a reliable index to more profound characters. Note, however, that, because a people is said to be civilized or literate, it does not follow that all its members can read and write, nor that they all lived in cities. Now there is no recorded instance of a community of savages civilizing themselves, adopting urban life or inventing a script. Wherever cities have been built, villages of preliterate farmers existed previously (save perhaps where an already civilized people have colonized uninhabited tracts). So civilization, wherever and whenever it arose, succeeded barbarism.

We have seen that a revolution as here defined should be reflected in the population statistics. In the case of the Urban Revolution the increase was mainly accounted for by the multiplication of the numbers of persons living together, i.e., in a single built-up area.

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The first cities represented settlement units of hitherto unprecedented size. Of course, it was not just their size that constituted their distinctive character. We shall find that by modern standards they appeared ridiculously small and we might meet agglomerations of population today to which the name city would have to be refused. Yet a certain size of settlement, and density of population, is an essential feature of civilization.

Now the density of population is determined by the food supply which in turn is limited by natural resources, the techniques for their exploitation and the means of transport and food-preservation available. The last factors have proved to be variables in the course of human history, and the technique of obtaining food has already been used to distinguish the consecutive stages termed savagery and barbarism. Under the gathering economy of savagery population was always exceedingly sparse. In aboriginal America the carrying capacity of normal unimproved land seems to have been from .05 to .10 per square mile. Only under exceptionally favourable conditions did the fishing tribes of the Northwest Pacific coast attain densities of over one human to the square mile. As far as we can guess from the extant remains, population densities in palaeolithic and pre-neolithic Europe were less than the normal American. Moreover such hunters and collectors usually live in small roving bands. At best several bands may come together for quite brief periods on ceremonial occasions such as the Australian corroborrees. Only in exceptionally favoured regions can fishing tribes establish anything like villages. Some settlements on the Pacific coasts comprised thirty or so substantial and durable houses, accommodating groups of several hundred persons. But even these villages were only occupied during the winter; for the rest of the year their inhabitants dispersed in smaller groups. Nothing comparable has been found in pre-neolithic times in the Old World.

The Neolithic Revolution certainly allowed an expansion of population and enormously increased the carrying capacity of suitable land. On the Pacific Islands neolithic societies today attain a density of 30 or more persons to the square mile. In pre-Columbian North America, however, where the land is not obviously restricted by surrounding seas, the maximum density recorded is just under 2 to the square mile.

Neolithic farmers could of course, and certainly did, live together in permanent villages, though, owing to the extravagant rural economy generally practised, unless the crops were watered by irrigation, the villages had to be shifted at least every twenty years. But on the whole the growth of population was not reflected so much in the enlargement of the settlement unit as in a multiplication of settlements. In ethnography neolithic villages can boast only a few hundred inhabitants (a couple of "pueblos" in New Mexico house over a thousand, but perhaps they cannot be regarded as neolithic). In prehistoric Europe the largest neolithic village yet known, Barkaer in Jutland, comprised 52 small, one-roomed dwellings, but 16 to 30 houses was a more normal figure; so the average local group in neolithic times would average 200 to 400 members.

These low figures are of course the result of technical limitations. In the absence of wheeled vehicles and roads for the transport of bulky crops men had to live within easy walking distance of their cultivations. At the same time the normal rural economy of the Neolithic Age, what is now termed slash-and-burnt or jhumming, condemns much more than half the arable land to lie fallow so that large areas were required. As soon as the population of a settlement rose above the numbers that could be supported from the accessible land, the excess had to hive off and found a new settlement.

The Neolithic Revolution had other consequences beside increasing the population, and their exploitation might in the end help to provide for the surplus increase. The new economy allowed, and indeed required, the farmer to produce every year more food than was needed to keep him and his family alive. In other words it made possible the regular production of a social surplus. Owing to the low efficiency of neolithic technique, the surplus produced was insignificant at first, but it could be increased till it demanded a reorganization of society.

Now in any Stone Age society, palaeolithic or neolithic, savage or barbarian, everybody can at least in theory make at home the few indispensable tools, the modest cloths and the simple ornaments everyone requires. But every member of the local community, not disqualified by age, must contribute actively to the communal food supply by personally collecting, hunting, fishing, gardening or herding. As long as this holds good, there can be no full-time specialists, no persons nor class of persons who depend for their livelihood on food produced by others and secured in exchange for material or immaterial goods or services.

We find indeed today among Stone Age barbarians

and even savages expert craftsmen (for instance, flint-knappers among the Ona of Tierra del Fuego), men who claim to be experts in magic, and even chiefs. In palaeolithic Europe too there is some evidence for magicians and indications of chieftainship in pre-neolithic times. But on closer observation we discover that today these experts are not full-time specialists. The Ona flint-worker must spend most of his time hunting; he only adds to his diet and his prestige by making arrowheads for clients who reward him with presents. Similarly a pre-Columbian chief, though entitled to customary gifts and services from his followers, must still personally lead hunting and fishing expeditions and indeed could only maintain his authority by his industry and prowess in these pursuits. The same holds good of barbarian societies that are still in the neolithic stage, like the Polynesians where industry in gardening takes the place of prowess in hunting. The reason is that there simply will not be enough food to go round unless every member of the group contributes to the supply. The social surplus is not big enough to feed idle mouths.

Social division of labor, save those rudiments imposed by age and sex, is thus impossible. On the contrary community of employment, the common absorption in obtaining food by similar devices guarantees a certain solidarity to the group. For co-operation is essential to secure food and shelter and for defence against foes, human and sub-human. This identity of economic interests and pursuits is echoed and magnified by identity of language, custom and belief; rigid conformity is enforced as effectively as industry in the common quest for food. But conformity and industrious co-operation need no State organization to maintain them. The local group usually consists either of a single clan (persons who believe themselves descended from a common ancestor or who have earned a mystical claim to such descent by ceremonial adoption) or a group of clans related by habitual intermarriage. And the sentiment of kinship is reinforced or supplemented by common rites focused on some ancestral shrine or sacred place. Archaeology can provide no evidence for kinship organization, but shrines occupied the central place in preliterate villages in Mesopotamia, and the long barrow, a collective tomb that overlooks the presumed site of most neolithic villages in Britain, may well have been also the ancestral shrine on which converged the emotions and ceremonial activities of the villagers below. However, the solidarity thus idealized and concretely symbolized, is really based on the same principles as that of a pack of wolves or a herd of sheep; Durkheim has called it "mechanical."

Now among some advanced barbarians (for instance, tattooers or woodcarvers among the Maori) still technologically neolithic we find expert craftsmen tending towards the status of full-time professionals, but only at the cost of breaking away from the local community. If no single village can produce a surplus large enough to feed a full-time specialist all the year round, each should produce enough to keep him a week or so. By going round from village to village an expert might thus live entirely from his craft. Such itinerants will lose their membership of the sedentary kinship group. They may in the end form an analogous organization of their own—a craft clan, which, if it remain hereditary, may become a caste, or, if it recruit its members mainly by adoption (apprenticeship throughout Antiquity and the Middle Age was just temporary adoption), may turn into a guild. But such specialists, by emancipation from kinship ties, have also forfeited the protection of the kinship organization which alone under barbarism, guaranteed to its members security of person and property. Society must be reorganized to accommodate and protect them.

In pre-history specialization of labor presumably began with similar itinerant experts. Archaeological proof is hardly to be expected, but in ethnography metal-workers are nearly always full-time specialists. And in Europe at the beginning of the Bronze Age metal seems to have been worked and purveyed by perambulating smiths who seem to have functioned like tinkers and other itinerants of much more recent times. Though there is no such positive evidence, the same probably happened in Asia at the beginning of metallurgy. There must of course have been in addition other specialist craftsmen whom, as the Polynesian example warns us, archaeologists could not recognize because they worked in perishable materials. One result of the Urban Revolution will be to rescue such specialists from nomadism and to guarantee them security in a new social organization.

About 5,000 years ago irrigation cultivation (combined with stock-breeding and fishing) in the valleys of the Nile, the Tigris-Euphrates and the Indus had begun to yield a social surplus, large enough to support a number of resident specialists who were themselves released from food-production. Water-transport, supplemented in Mesopotamia and the Indus valley by

wheeled vehicles and even in Egypt by pack animals, made it easy to gather food stuffs at a few centres. At the same time dependence on river water for the irrigation of the crops restricted the cultivable areas while the necessity of canalizing the waters and protecting habitations against annual floods encouraged the aggregation of population. Thus arose the first cities—units of settlement ten times as great as any known neolithic village. It can be argued that all cities in the old world are offshoots of those of Egypt, Mesopotamia and the Indus basin. So the latter need not be taken into account if a minimum definition of civilization is to be inferred from a comparison of its independent manifestations.

But some three millennia later cities arose in Central America, and it is impossible to prove that the Mayas owed anything directly to the urban civilizations of the Old World. Their achievements must therefore be taken into account in our comparison, and their inclusion seriously complicates the task of defining the essential preconditions for the Urban Revolution. In the Old World the rural economy which yielded the surplus was based on the cultivation of cereals combined with stock-breeding. But this economy had been made more efficient as a result of the adoption of irrigation (allowing cultivation without prolonged fallow periods) and of important inventions and discoveries—metallurgy, the plough, the sailing boat and the wheel. None of these devices was known to the Mayas; they bred no animals for milk or meat; though they cultivated the cereal maize, they used the same sort of slash-and-burn method as neolithic farmers in prehistoric Europe or in the Pacific Islands today. Hence the minimum definition of a city, the greatest factor common to the Old World and the New will be substantially reduced and impoverished by the inclusion of the Maya. Nevertheless ten rather abstract criteria, all deducible from archaeological data, serve to distinguish even the earliest cities from any older or contemporary village.

(1) In point of size the first cities must have been more extensive and more densely populated than any previous settlements, although considerably smaller than many villages today. It is indeed only in Mesopotamia and India that the first urban populations can be estimated with any confidence or precision. There excavation has been sufficiently extensive and intensive to reveal both the total area and the density of building in sample quarters and in both respects has disclosed significant agreement with less industrialized Oriental cities today. The population of Sumerian cities, thus calculated, ranged between 7,000 and 20,000; Harappa and Mohenjo-daro in the Indus valley must have approximated to the higher figure. We can only infer that Egyptian and Maya cities were of comparable magnitude from the scale of public works, presumably executed by urban populations.

(2) In composition and function the urban population already differed from that of any village. Very likely indeed most citizens were still also peasants, harvesting the lands and waters adjacent to the city. But all cities must have accommodated in addition classes who did not themselves procure their own food by agriculture, stock-breeding, fishing or collecting—full-time specialist craftsmen, transport workers, merchants, officials and priests. All these were of course supported by the surplus produced by the peasants living in the city and in dependent villages, but they did not secure their share directly by exchanging their products or services for grains or fish with individual peasants.

(3) Each primary producer paid over the tiny surplus he could wring from the soil with his still very limited technical equipment as tithe or tax to an imaginary deity or a divine king who thus concentrated the surplus. Within this concentration, owing to the low productivity of the rural economy, no effective capital would have been available.

(4) Truly monumental public buildings not only distinguish each known city from any village but also symbolize the concentration of the social surplus. Every Sumerian city was from the first dominated by one or more stately temples, centrally situated on a brick platform raised above the surrounding dwellings and usually connected with an artificial mountain, the staged tower or ziggurat. But attached to the temples, were workshops and magazines, and an important appurtenance of each principal temple was a great granary. Harappa, in the Indus basin, was dominated by an artificial citadel, girt with a massive rampart of kiln-baked bricks, containing presumably a palace and immediately overlooking an enormous granary and the barracks of artisans. No early temples nor palaces have been excavated in Egypt, but the whole Nile valley was dominated by the gigantic tombs of the divine pharaohs while royal granaries are attested from the literary record. Finally, the Maya cities are known almost exclusively from the temples and pyramids of sculptured stone round which they grew up.

Hence in Sumer the social surplus was first effectively concentrated in the hands of a god and stored in his granary. That was probably true in Central America while in Egypt the pharaoh (king) was himself a god. But of course the imaginary deities were served by quite real priests who, besides celebrating elaborate and often sanguinary rites in their honour, administered their divine masters' earthly estates. In Sumer indeed the god very soon, if not even before the revolution, shared his wealth and power with a mortal viceregent, the "City-King," who acted as civil ruler and leader in war. The divine pharaoh was naturally assisted by a whole hierarchy of officials.

(5) All those not engaged in food-production were of course supported in the first instance by the surplus accumulated in temple or royal granaries and were thus dependent on temple or court. But naturally priests, civil and military leaders and officials absorbed a major share of the concentrated surplus and thus formed a "ruling class." Unlike a palaeolithic magician or a neolithic chief, they were, as an Egyptian scribe actually put it, "exempt from all manual tasks." On the other hand, the lower classes were not only guaranteed peace and security, but were relieved from intellectual tasks which many find more irksome than any physical labor. Besides reassuring the masses that the sun was going to rise next day and the river would flood again next year (people who have not five thousand years of recorded experience of natural uniformities behind them are really worried about such matters!), the ruling classes did confer substantial benefits upon their subjects in the way of planning and organization.

(6) They were in fact compelled to invent systems of recording and exact, but practically useful, sciences. The mere administration of the vast revenues of a Sumerian temple or an Egyptian pharaoh by a perpetual corporation of priests or officials obliged its members to devise conventional methods of recording that should be intelligible to all their colleagues and successors, that is, to invent systems of writing and numeral notation. Writing is thus a significant, as well as a convenient, mark of civilization. But while writing is a trait common to Egypt, Mesopotamia, the Indus valley and Central America, the characters themselves were different in each region and so were the normal writing materials—papyrus in Egypt, clay in Mesopotamia. The engraved seals or stelae that provide the sole extant evidence for early Indus and Maya writing, no more represent the normal vehicles for the scripts than do the comparable documents from Egypt and Sumer.

(7) The invention of writing—or shall we say the inventions of scripts—enabled the leisured clerks to proceed to the elaboration of exact and predictive sciences—arithmetic, geometry and astronomy. Obviously beneficial and explicitly attested by the Egyptian and Maya documents was the correct determination of the tropic year and the creation of a calendar. For it enabled the rulers to regulate successfully the cycle of agricultural operations. But once more the Egyptian, Maya and Babylonian calendars were as different as any systems based on a single natural unit could be. Calendrical and mathematical sciences are common features of the earliest civilizations and they too are corollaries of the archaeologists' criterion, writing.

(8) Other specialists, supported by the concentrated social surplus, gave a new direction to artistic expression. Savages even in palaeolithic times had tried, sometimes with astonishing success, to depict animals and even men as they saw them—concretely and naturalistically. Neolithic peasants never did that; they hardly ever tried to represent natural objects, but preferred to symbolize them by abstract geometrical patterns which at most may suggest by a few traits a fantastical man or beast or plant. But Egyptian, Sumerian, Indus and Maya artist-craftsmen—full-time sculptors, painters, or seal-engravers—began once more to carve, model or draw likenesses of persons or things, but no longer with the naive naturalism of the hunter, but according to conceptualized and sophisticated styles which differ in each of the four urban centres.

(9) A further part of the concentrated social surplus was used to pay for the importation of raw materials, needed for industry or cult and not available locally. Regular "foreign" trade over quite long distances was a feature of all early civilizations and, though common enough among barbarians later, is not certainly attested in the Old World before 3,000 B.C. nor in the New before the Maya "empire." Thereafter regular trade extended from Egypt at least as far as Byblos on the Syrian coast while Mesopotamia was related by commerce with the Indus valley. While the objects of international trade were at first mainly "luxuries," they already included industrial materials, in the Old World notably metal the place of which in the New was perhaps taken by obsidian. To this extent the first cities were dependent for vital materials on long distance

trade as no neolithic village ever was.

(10) So in the city, specialist craftsmen were both provided with raw materials needed for the employment of their skill and also guaranteed security in a State organization based now on residence rather than kinship. Itinerancy was no longer obligatory. The city was a community to which a craftsman could belong politically as well as economically.

Yet in return for security they became dependent on temple or court and were relegated to the lower classes. The peasant masses gained even less material advantages; in Egypt for instance metal did not replace the old stone and wood tools for agricultural work. Yet, however imperfectly, even the earliest urban communities must have been held together by a sort of solidarity missing from any neolithic village. Peasants, craftsmen, priests and rulers form a community, not only by reason of identity of language and belief, but also because each performs mutually complementary functions, needed for the well-being (as redefined under civilization) of the whole. In fact the earliest cities illustrate a first approximation to an organic solidarity based upon a functional complementarity and interdependence between all its members such as subsist between the constituent cells of an organism. Of course, this was only a very distant approximation. However necessary the concentration of the surplus really were with the existing forces of production, there seemed a glaring conflict on economic interests between the tiny ruling class, who annexed the bulk of the social surplus, and the vast majority who were left with a bare subsistence and effectively excluded from the spiritual benefits of civilization. So solidarity had still to be maintained by the ideological devices appropriate to the mechanical solidarity of barbarism as expressed in the pre-eminence of the temple or the sepulchral shrine, and now supplemented by the force of the new State organization. There could be no room for sceptics or sectaries in the oldest cities.

These ten traits exhaust the factors common to the oldest cities that archaeology, at best helped out with fragmentary and often ambiguous written sources, can detect. No specific elements of town planning for example can be proved characteristic of all such cities; for on the one hand the Egyptian and Maya cities have not yet been excavated; on the other neolithic villages were often walled, an elaborate system of sewers drained the Orcadian hamlet of Skara Brae; two-storeyed houses were built in pre-Columbian *pueblos*, and so on.

The common factors are quite abstract. Concretely Egyptian, Sumerian, Indus and Maya civilizations were as different as the plans of their temples, the signs of their scripts and their artistic conventions. In view of this divergence and because there is so far no evidence for a temporal priority of one Old World centre (for instance, Egypt) over the rest nor yet for contact between Central America and any other urban centre, the four revolutions just considered may be regarded as mutually independent. On the contrary, all later civilizations in the Old World may in a sense be regarded as lineal descendants of those of Egypt, Mesopotamia or the Indus.

But this was not a case of like producing like. The maritime civilizations of Bronze Age Crete or classical Greece for example, to say nothing of our own, differ more from their reputed ancestors than these did among themselves. But the urban revolutions that gave them birth did not start from scratch. They could and probably did draw upon the capital accumulated in the three allegedly primary centres. That is most obvious in the case of cultural capital. Even today we use the Egyptians' calendar and the Sumerians' divisions of the day and the hour. Our European ancestors did not have to invent for themselves these divisions of time nor repeat the observations on which they are based; they took over—and very slightly improved systems elaborated 5,000 years ago! But the same is in a sense true of material capital as well. The Egyptians, the Sumerians and the Indus people had accumulated vast reserves of surplus food. At the same time they had to import from abroad necessary raw materials like metals and building timber as well as "luxuries." Communities controlling these natural resources could in exchange claim a slice of the urban surplus. They could use it as capital to support full-time specialists—craftsmen or rulers—until the latters' achievement in technique and organization had so enriched barbarian economies that they too could produce a substantial surplus in their turn.

The Natural History of Urbanism

ROBERT McC. ADAMS

The underlying bond between cities and their hinterlands is that the existence of the former depends upon their capacity to mobilize and deploy the latter's agricultural surpluses. Perhaps this is a truism, but like all truisms it obscures a complex reality. Does it imply that major steps in urban development have depended upon prior improvements in agricultural productivity rather than vice versa? What meaning can be ascribed to the concept of an agricultural surplus that is independent of the social system in which it is voluntarily brought forward as offerings, forcibly extracted as taxes, or exchanged for other goods and services in an urban market? How much, in fact, of the whole range of interactions between city and countryside is accurately epitomized by a statement of their purely economic relationship?

Merely to ask these questions evokes a less constrictive approach. To understand the origins and evolution of urban centers, including the diversity of their institutional arrangements and physical forms, we must deal with city and countryside not only as opposed abstractions on an economic plane but as intimately interacting parts of an embracing cultural and ecological system.

Granting that such an undertaking may be of some historical or philosophical interest, what relevance does it have today as we grope for solutions to an unprecedented urban crisis? One argument, of course, is that the past continues to exercise a subtle but pervasive influence upon our perception of present realities. In this sense, as Benedetto Croce observed, all history is contemporary history. It must be studied if the patterns of thought and action we inherit are to be understood or even recognized and, in any case, these patterns cannot simply be excised at will from our present lives.

Equally important, the technological destruction of distance that characterizes our epoch is rapidly tending to fuse all of the metropolises of the world into instances of a single type. Hence some thought about earlier cities that were less closely in interaction with one another may help to clarify the basic attributes of cities generally. If we stress the positive and negative features of urbanism as a broad category of settlement and adaptation, features which in some cases have preserved an uneasy balance over millenniums, it may help us also to modify the sense of complete and wasteful novelty with which all too frequently current problems are regarded.

In long-term evolutionary perspective, the growth of cities very closely followed the introduction of agriculture. No more than four to six millenniums seem to have separated the first reliance on domesticates—for even a minor proportion of the diet—from the emergence of settlements whose size and complexity unambiguously attest to their full urban status. In contrast to the many hundreds of millenniums of man's earlier biological and cultural development, this is a relatively insignificant interval. About the same interval, it might be noted, separated the Urban Revolution from the Industrial Revolution. Industrialism followed and was dependent upon certain concomitants of urban life: the accumulative growth of technology, the elaboration of economic systems permitting the support of craft specialists, and the appearance of a class of entrepreneurs able to mobilize capital for ends not previously sanctioned. It is hardly very useful, however, to say that the latter

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development was a *consequence* of the former. In the same way, the Food-Producing and the Urban Revolutions also must be distinguished from one another, even while we recognize that the order in which they occurred was a necessary and inevitable one.

What is more important about both agriculture and urbanism is that both originated independently in a number of widely separated centers in the Old and New Worlds rather than diffusing outward from a single source. This complicates the task of generalizing about their interrelationships, in that both similarities and differences between the separate instances need to be taken into account. On the other hand, the fact that there were essentially independent sequences of change culminating in roughly similar institutional arrangements highlights regularities in the processes of change and lends importance to the search for causal explanations of them. A systematic analysis of these regularities is severely limited, to be sure, by the narrowly specialized concerns and inherent obscurity of the early written sources. Prior to the advent of writing—generally, but not in all cases, closely associated with the onset of urban civilization itself—we are confronted with the still more severe shortcomings of a purely archeological record. Given such evidence, the fact that there were indisputable regularities is not as helpful as it might seem. Only an irrepressible optimist would assume that wide consensus and a real sense of closure are soon to be realized on even the basic processes involved.

Within the limitations of our data, conditions antecedent to the first appearance of urban centers can be quickly sketched. Although the beginnings of agriculture closely followed the end of the Pleistocene, they cannot be explained as merely the consequence of a new set of environmental conditions. To be sure, the introduction and spread of agricultural techniques was almost explosively far-reaching and rapid in comparison with the earlier, almost imperceptible, pace of increasing hunting-gathering efficiency and cultural complexity. Yet all the potential domesticates were confined to regions far removed from the marked climatic and life-zone changes associated with the advance and retreat of the glaciers. And such environmental changes as there were, differed little from those that occurred repeatedly during earlier, warmer intervals of the Pleistocene. Having crossed some ill-defined threshold of complexity, what was apparently different some nine or ten thousand years ago was man's capacity to rapidly elaborate new responses to long pre-existing environmental potentials.

Examples of this enhanced adaptive capacity can be found in many regions of both the Old and New Worlds. Perhaps the widest and most easily observed development was in the direction of increasing sedentism. Particularly favored were environmental niches in which different food resources complemented one another, permitting substantial enlargement of the local group through the full turn of seasons while at the same time reducing dependence upon migration. Technical innovations also played a part. While naturally there were differences from region to region, such improvements regularly included composite tools and weapons: the bow and arrow; ground stone utensils appropriate for carpentry, food preparation, and other uses; and new devices for transport with a potential importance by no means confined to the food quest.

The independent origins of agriculture in the Near East, Mesoamerica, and probably other regions can be thought of as manifestations of these widely occurring developments under circumstances in which the local biota included species of plants and animals that could be domesticated and utilized with the foraging, extractive, and culinary technology at hand. Retrospectively, the line between hunting or harvesting wild resources and consciously planting or breeding them appears to us as a Rubicon, but the portents of crossing it must not have been apparent to those who first did so. In the early post-Pleistocene milieu of increasingly assured manipulation and control of local food resources of all kinds, the elaboration of techniques we deem agricultural must have taken the form of intensified experimentation and a resultant sequence of small, locally variable improvements along already familiar lines.

The major initial effect of an agricultural mode of subsistence was the extension of the zones of settled life far beyond the restricted niches in which the potential domesticates originally were at home. Spreading outward from numerous local centers for particular species, such zones soon overlapped extensively. Agriculture was rendered more secure as a mode of subsistence through this increase in the varieties of domestic resources, and its seasonal cycle and requisite techniques were increasingly differentiated from those for hunting and gathering. The extent of the divergence between early agricultural economies and their precursors, however, should not be overestimated. Given

still primitive techniques, the limited caloric value of many of the early cultigens, and almost uncounted natural hazards, the relative security and productiveness that we associate with agriculture was not within reach. As a way of life it was perhaps more continuously demanding than hunting and gathering, with the participation of all but the very young and very old being required in subsistence pursuits. Percentage calculations of dietary intake are possible only in rare instances, but the general pattern clearly was one of a necessary, continuing reliance on a broad spectrum of domesticates, weeds of cultivation, and naturally occurring foods.

With the establishment of a settled, agricultural, way of life, a new set of ecological processes can be discerned. To generalize from the Mesopotamian and Mesoamerican cases in which the data are most abundant, the regions of earliest urban growth were *not* identical with those in which agriculture had originated. One of the essential features of this geographical shift probably was the development of more intensive agricultural techniques for which previously marginal areas now became optimal, although we remain very poorly informed about the nature and timing of major innovations like irrigation, complementary planting of mixed crops to assure fertility, and the plow. Such innovations would permit the formation of greater food surpluses by individual agricultural producers. Perhaps more important, they also freed certain members of the community for at least part-time specialization and encouraged the accumulation of stores with which to meet periodic shortages.

On this basis a trend toward increasing population seems fairly certain. It is difficult, however, to confirm such a trend from the limited and indirect evidence of changing settlement patterns and the minuscule portions of archeological sites that generally are excavated. Thus, all that can be said with confidence is that the frontiers of agriculture continued to expand rapidly. And at the frontiers, of course, we must reckon not only with natural increases in population resulting from agriculture but also with the direct conversion to agriculture of former hunter-gatherers.

Perhaps the most reasonable demographic reconstruction at present is that, while there was some increase in population density generated by the shift to more intensive forms of cultivation, the scarce resource in early civilizations generally remained people rather than land. Accordingly, movement was relatively easy and there was little demographic inducement for the emergence of stable polities. Periodic reshufflings prevented the sharp polarization of society into small groups exercising a tight monopoly of productive resources and the resultant reduction of other groups to the permanent status of dependent retainers. If this is so, other factors must have been the primary motive forces behind the formation of early states: chronic inequalities in productivity between different regions; the selectively impoverishing effects of the numerous natural hazards to agricultural subsistence; the accumulative growth of redistributive institutions once they had passed beyond a certain critical threshold of size; and, perhaps, the military advantages conferred on those types of social organization best able to equip and mobilize large bodies of militia or professional soldiery.

Viewed from the standpoint not of labor productivity but of land productivity, the effects of agricultural intensification may have been equally significant. Means of transport remained primitive; in most of the New World only human portage was available, but even in the Near East the economic use of wheeled vehicles apparently followed the appearance of urban civilization only after a long interval. Hence, long-distance movements were confined to strategic raw materials and luxuries. Those agricultural hinterlands whose surpluses could be economically brought in to sustain the growth of population centers were of very limited radius. Techniques leading to increased output per unit area therefore directly permitted an increase in settlement size.

In short, increased size and density were perhaps the crucial characteristics of early urban centers. Numerous general treatises on cities notwithstanding, there is no evidence that the onset of urbanization was accompanied—much less caused—by rapid, significant changes in the prevailing division of labor. The overwhelming proportion of the population continued for some time to be engaged primarily in agricultural pursuits, as was still the case until very recently with many African cities. Congregated in large settlements, city dwellers were more easily subject to political control, taxation, military service, and corvee labor, but the contribution of specialized, urban crafts to the satisfaction of their primary needs remained extremely small. The small handfuls of craftsmen, scribes, and other specialists found in early cities were, for the most part, in the service of the major

institutions and were employed primarily in the production of military equipment and ritual articles required for cult observances.

There was another feature of the newly emergent, intensive agricultural regimes which was central to the ensuing growth of urban societies. The areas in which independent civilizations originated seem to have been characterized by a series of specialized microenvironments for which different, equally specialized, subsistence pursuits were appropriate. Such was the case, for example, with the lateral succession of fishing, farming, and herding as one moved away from the major watercourses in the Near East, or with the replacement of one crop complex by another with increasing altitudes in the Mesoamerican and Andean highlands. The existence of interdependent zones of this type fostered the formation of "symbiotic regions" within which the complementary distribution of subsistence products could be effected. To deal with this new order of complexity, it is not surprising that writing systems were invented or independently elaborated, in turn permitting further increases in administrative complexity and lending formality and continuity to urban traditions. To the extent that these developments contributed to the formation of centralized institutions, the personnel and facilities of the latter could only contribute to the further growth of the principal settlements in which they were located.

The tempo and sequence of these ecological trends in relation to the onset of urban life still is largely obscure. There are at least a few well-documented cases to suggest that the initial growth of cities was generally rapid, sometimes even being the outcome of conscious policies applied within a single generation. Hence the achievement of the forms of civilized, urban life was relatively sudden; nevertheless, the fashioning and consolidation of the base that could support an urban superstructure may have been a much longer process. Almost certainly, for example, there were mechanisms of interzonal exchange and redistribution available and functioning on some level before new urban elites arose to redirect and administer them. To judge from comparable societies studied by recent ethnographers, somewhat similar purposes can be served by a variety of devices that do not presuppose the submission of scattered communities of agriculturalists to some paramount leadership. Bride-wealth payments linking exogamous, differently specialized communities are among such devices. So also are cycles of cult observances in shifting localities, which provide not only for the movement of religious ideas and pilgrims but also of goods.

In addition, there was undeniably at least some ordinary trade in items that were hardly luxuries long antedating the appearance of cities; obsidian for the manufacture of finely chipped stone tools is a widely occurring case in point. "Trade" in this sense, however, is a gross, essentially uninformative term that does nothing to clarify the relationship between the participating prehistoric communities or the manner in which such relationships were affected by the subsequent rise of urban centers. Fortunately, recent advances in archeological methodology suggest that we can soon move beyond the mere recognition of exotic materials to a quantitative analysis of some of the components of social behavior by which that material was circulated.

It may be useful at this point to restate the ecological role of early cities at the time of their origins. They seem fairly generally to have functioned as junction points or nodes in the appropriation and redistribution of agricultural surpluses. In addition, they provided a permanent base for the operation of new institutions that no longer merely mediated but, instead, authoritatively administered the interrelationships between specialized producers occupying adjacent econiches. Such institutions were embedded in realms of cultural meaning not confined to their primary ecological functions, including attempts on many levels to unify, symbolize, and stabilize the newly emergent, urban-dominated social patterns. Cities became focal points not merely for the safe storage of surpluses prior to their deployment, but for conspicuous expenditures for public building programs, for the maintenance of elites in luxurious surroundings, and for the enhancement of military power. With the concentration of wealth, early urban centers became both proponents of expansionism and powerful incentives for external attack. Massive fortifications accordingly became one of their dominant architectural forms.

A summary like the foregoing is necessarily abstract and generalized. It emphasizes recurrent features at the expense of known variations and lacunae in the evidence, hence perhaps seeming to imply that early cities everywhere were the outcome of a tightly interconnected, uniform set of causal processes. It may be taken to imply further that the superior adaptive potential of the city led in some direct fashion to its origin and contributed decisively to the spread of

urbanism. What could be more advantageous, after all, than state-protected, rationally allocated stores of surpluses to compensate for year-to-year fluctuations in harvest? Or centralized investment in and control of irrigation canals and other facilities for intensive agriculture and the improvement of transport?

Although perhaps not incorrect at some fairly high level of abstraction, both implications are misleading. The subsequent growth and spread of cities cannot be understood as the irresistible sweep of a set of innovations which were obviously superior in socioeconomic terms. Instead, the process by which cities assumed the ecologically dominant role we now associate with them consisted of a shifting, complex, quite unstable, adjustment of environmental, economic, technological, political, and even ideological factors. Cumulatively, of course, the trend was irrevocably toward urbanism, but contained within this worldwide trend extending over millenniums were repeated advances and reverses at the local level. To understand this irregularity we must consider not just later increments in urbanism but also some of the attendant costs and dangers.

Perhaps the most informative single index of the subsequent evolution of cities is the increase in size. Early direct testimony on this is notoriously untrustworthy, so that historical demographers must depend on the convergence of secondary, approximate, probabilistic lines of reasoning. Even the order of magnitude of the population of particular cities often remains a matter of sharp debate, although there is broad agreement that older claims of immense size must be viewed with increasing skepticism. As a generalization for the Old World, probably no city was larger than several tens of thousands of inhabitants before the first millennium B.C., while by the Middle Ages cities of several hundreds of thousands occurred at intervals in the Mediterranean basin and across Asia to China. The picture is somewhat more obscure in the civilized areas of the aboriginal New World; the earlier stage certainly had been attained by not long after the time of Christ, while the onset of the second stage may have been close at hand when the independent sequence of development was ended by the Spanish Conquest.

Undoubtedly these parallel, decisive increases reflect improvements in the subsistence economy, but it is important to note that any such improvements rested on technical innovations only to a limited degree. In the aboriginal New World, in fact, crop complexes and methods of cultivation both seem to have remained remarkably stable. The Andean area offers partial exceptions, but at least in Mesoamerica there is no evidence for any significant advance in the technology or organization of agriculture from remote precivilized times until the flourishing urban societies so vividly described by the Spaniards.

To be sure, some new, specialized crops like sugar and silk were brought under cultivation in the Old World, many of them for the first time of a highly labor-intensive character suitable to increasing populations. With the exception of the westward spread of irrigated rice-lands, however, few of the new items became subsistence staples. The harnessing of wind and water power for rotary movement was important in the spread of agriculture into arid areas and in reducing the costs of milling and similar operations, but its direct contribution to urban growth was hardly a major one.

The introduction of iron presents a somewhat more complicated case, although not a basically dissimilar one. With the rapidly spreading adoption of iron during the first millennium B.C., into the hands of agriculturalists came implements that were not only more durable than bronze but that could be produced much more cheaply and from more widely scattered bodies of ore. The availability of iron, however, led only very slowly to the development of more efficient tools and processing techniques. Probably its primary effects on urban life were associated instead with its low cost. Iron tools now became available to ordinary workmen deployed in large numbers on major public works directed towards the extension and improvement of agricultural lands. Partly as a consequence, the intensity of agriculture in the neighborhood of cities was increased through the formation of encircling "green belts" of irrigated, continuously cultivated orchards and truck gardens. Some areas of formerly limited agricultural potential also were transformed, including the Iranian plateau and parts of North Africa, with corresponding effects upon their political importance.

If the immediate hinterlands of cities were not converted by this process into metropolitan areas in the modern sense, they were nonetheless integrated into an urban-centered society to an unprecedented degree. To take only economic features for which the evidence is least ambiguous and most durable, there was a—not steady but, on the whole, cumulative—reduction in the self-sufficiency of even

outlying peasant communities. To the eye of the reconnoitering archeologist, for example, a litter of iron, glazed pottery sherds, glass, and fired brick fragments, immediately distinguishes Near Eastern sites of all sizes occupied after roughly the time of Alexander from any earlier ones. All of these remains were the products of urban specialists which, after more than a millennium of having been largely restricted to official or luxury use, passed into general circulation with dramatic suddenness and irreversible effect. Coinage was perhaps the most important of the widely circulated new features, and it was also the only recent innovation among them. Its crucial significance lay not in any contribution to advancing technology but in the networks of economic interrelations that it facilitated: it was through these networks that other craft products reached wider, peasant markets.

As this suggests, the increase in urban size by a full order of magnitude was primarily a reflection of developments in political economy rather than in technology. The earlier, territorially consolidated unit was the city state, which either maintained itself as an island in a barbarian sea or else contended periodically with neighboring units of the same kind within a framework of regional rivalries and ephemeral alliances. In time, larger administrative units—of which the Third Dynasty of Ur and the Dynasty of Hammurabi in Babylon (and perhaps the Egyptian Old Kingdom, at an even earlier period) are good examples—began to make their appearance. Wider conquests were extended outward from a firmly pacified if not unified heartland, until, after a few generations, the thinness of the centralized administrative veneer became apparent. Then the old fissures reopened in the home territory and the assertions of hegemony over distant areas gradually were dropped.

By contrast, the great cities of classical and later times characteristically came into being as components of large, relatively long-lived, continental empires. Such cities were creatures of strong patrimonial regimes to a degree quite unmatched earlier. They directly depended on royal largesse for the support of the now predominant proportion of artisans, tradesmen, soldiers, and petty officials. Not infrequently, royal intervention was even more tangibly reflected in their planned, overall layouts.

As part of a wider spectrum of efforts to maintain or enlarge the central power and to prevent its devolution into the hands of a newly emergent landed nobility, these later cities were founded, manipulated, and (at times) even abandoned almost by royal whim. Behind numerous instances of capricious exercise of power, however, lay conscious policies of shifting populations to destroy parochial loyalties and of strengthening the administrative fabric over the whole realm. Unprecedented emphasis was attached to improvements in communications, with the development of "Royal Roads," post routes, and local garrisons and caravansaries to assure security of movement. With the relative success of these policies for long periods, warfare for the most part could be limited to distant frontiers with barbarians or to zones of contention with rival powers of like magnitude. This, in turn, permitted more intensive, long-range development of the vital central regions of empires, aided by the flow of refugees and captives inward from the frontiers.

Under circumstances like these, the ecology of cities no longer can be understood in terms of their immediate sustaining areas. By medieval times, many individual Old World cities were widely identified with particular specialized products—Damascus blades, Mosul muslins, Bokhara carpets, Venetian silks—confirming the existence of highly institutionalized, long-distance trade for which the stimulus and setting had become a genuinely international one. Rome in the first century A.D. was probably exceptional, in that a third of its wheat had to be shipped in from Egypt. To judge from studies of relatively recent examples of the same kind, virtually all of the necessary food supplies for most other preindustrial cities continued to be drawn from very limited, adjacent areas. But the raison d'être of all these imperial centers, as well as the explanation for the periodic rise and decline in their fortunes, lay in the policies and problems of patrimonial regimes which, like the cities themselves, had attained a new order of size.

We must return, however, to the price that had to be paid for these achievements. To begin with, for all but the most recent chapter of urban history, cities have not been sources of population growth but of severe population loss. It is uncertain whether preindustrial urban life was normally associated with lowered birth rates, but there is no doubt that urban mortality rates were much higher than rural ones prior to the very recent introduction of effective measures for public health. The increased density and aggregate size of settlement, in relation to primitive techniques of sanitation and other means of controlling vectors of

disease, could only lead not just to greater periodic losses in epidemics but also to heightened mortality, particularly of infants and children, as a regularly prevailing feature. The growth of cities, and even their continuing existence, accordingly was always dependent on an inflow of population from rural districts and smaller towns.

The founding or expansion of cities that was repeatedly boasted of by strong rulers in antiquity could have been either an expression of the accelerated movement of urban immigrants in search of the prosperity that followed in the wake of conquest, or a consequence of the extension of more coercive forms of royal control over the countryside. Neither alternative substantially affected the urban-rural differences in mortality rates, except in the sense that exceptionally short-sighted and acquisitive regimes could compensate to a degree by substantially depressing the living standards of the rural peasantry. Attitudes of conscious and sustained encouragement for rural well-being on the whole are distinguished in historical records by their rarity, seldom occurring except at times of unusual political stability. Under most circumstances, there were practical difficulties in drawing off so large a surplus that the agricultural producers were reduced to or below a bare subsistence margin. But it is noteworthy that, at least in the Near East, general movement in time of famine was toward cities and not away from them. With absentee landlords and predatory officials, as well as with sustained high levels of banditry and periodic nomadic incursions, it would appear from this that reserves of food adequate to meet prolonged crises seldom could be accumulated in smaller settlements.

While the concentration of surpluses in urban centers brought some relative immunity from fluctuations in the harvest, this boon frequently was accompanied by an exposure to new perils. Traditions associated with urban institutions have a force and continuity of their own, limiting the mobility of city populations in response to disastrous changes in local environmental conditions. Any tendency toward increased full-time specialization in nonagricultural pursuits, of course, would have the same effect. Intensified urban pressure on agricultural resources can, and in some important cases evidently did, lead to soil exhaustion or, as a consequence of over-irrigation, to crippling increases in soil salinity. The substitution of monocrop cultivation for diversified agriculture, apparently as a dependent rural extension of urban, market-oriented economies, is widely known for its attendant impoverishment of both the land and the husbandman. Policies of excessive taxation made necessary by overextended urban establishments sometimes led to the abandonment of villages and farmlands, further accentuating the imbalance between rural capacities and urban demands.

There were drawbacks even where the positive contribution of the state seems most apparent. For example, the substitution of large-scale, state-run irrigation works for smaller, locally maintained systems enlarged the agricultural base on which cities could depend and placed new powers of control in the hands of their elites. However, because such systems were beyond the capacity of local agriculturalists to administer and maintain, they were dependent on the—altogether unlikely—permanent conjunction of urban economic strength, political stability, and favorable attitudes toward rural investment. Moreover, the truly large-scale systems of the last twenty-five hundred years or so, through the increased supplies of water they assured and their disruption of natural drainage patterns, vastly increased the dangers of salinization and consequent land abandonment.

What all of these processes have in common is the increased systemic fragility to which they led. Perhaps the clearest, most dismal reflection of that fragility was that both rural and urban population curves apparently were characterized by a succession of marked peaks and troughs, quite in contrast to the steady and rising curves of the last few centuries. The approach of an urban ecological climax, in other words, was only attained—prior to the Industrial Revolution—at the cost of a dangerous narrowing of ecological alternatives.

"Rural" and "urban" generally are employed as polar opposites, but the examples just cited suggest that from an ecological standpoint this is seriously misleading. Both categories represent a conflation of related and unrelated features, not all of which correspond neatly to the more or less assumed gross differences between the two. In fact, it would appear that the most essential characteristic of urban and rural adaptations was not their mutual isolation but their historic complementarity and interdependence. At best, they are somewhat arbitrarily defined components of a single, embracing, cultural-environmental system, each changing only in close response to changes in the other. The dichotomy between "rural" and "urban" which still persists in our

thinking probably reflects an even deeper failure to understand that developments both in city politics and subsistence economics ultimately converge in their effects upon the success or failure of a society's whole pattern of adaptive responses to its environmental setting. Embodied in written traditions that formerly were largely limited to and inculcated by urban elites unfamiliar with agriculture, this misunderstanding continues to exert a negative influence on academic judgment and administrative policy.

The special biases and limitations of written traditions preserved by urban elites have seriously distorted our understanding of the historical ecology of cities in a number of other respects. Most important is the narrowness of the recorded tradition itself, with its concentration on urban politics and state administrative records, cults devoted to city deities, and military struggles between rival polities. From such sources, very little can be learned of the relationship between town and countryside, since what lay outside the walls simply was not felt to be worthy of notice. This bias persists even in the accounts of literate travelers familiar with rural districts. For example, there are many descriptions of ancient and medieval Near Eastern cities, often quite detailed and almost modern in their objective quality, but it is not until the European accounts of the last few centuries that we begin to find commonplace references to conditions in smaller towns and villages along the caravan routes between the major centers.

To make matters worse, this absence of written reference to rural conditions has been accentuated by the concentration of archeological excavations in the same major centers from which the textual sources stem. Until quite recently the principal motivation for archeological research on historic periods has been the finding of monumental buildings and tombs, with the hoped-for association of luxury goods and written records. Fortunately, this objective has now broadened in conformity with the interests of the social sciences: there is an awakening interest in regional studies, for example, in which entire settlement patterns are scrutinized with the aid of sampling procedures. But it will be many years before archeological data from minor settlements begin to permit an understanding of their nature and importance.

Even insofar as early urban chronicles are indirectly relevant to their wider, extramural context, there are serious distortions somehow to be overcome. Literary stereotypes abound, with the individual husbandman regarded patronizingly as ignorant and docile while the aggregate of rural dwellers, on their own ground, were pictured as brutish and terrifying. Such stereotypes, lumping together diverse interests and modes of life, conceal important roots of change in rural society. To some degree, they probably also overstress its passivity in contrast to urban initiative. Not unnaturally, mention was more likely to be made of sporadic urban contributions to rural well-being than to continuing interrelations which prevailingly ran the other way. Responsibility is claimed repeatedly for the initiation of irrigation projects, for example, but their subsequent neglect by urban administrators seldom receives mention and the assignment of blame for their abandonment solely to external enemies usually is more than doubtful. Only the rare and usually ineffectual reformer speaks of the oppressive effects upon the countryside of the manipulation of urban credit. Yet in the dangerously uncertain world of the peasant, the need for credit was endlessly renewed and always vital.

The traditional character of literary sources deriving from largely illiterate societies leads to assertions of continuity which are also doubtful. Neither changes in basic institutional patterns nor major cycles of urban growth and abandonment penetrated this stream of recorded consciousness in any necessary proportion to their effects upon the society at large. Furthermore, there is always a problem with observations or statements which applied not to whole regions or countries but to privileged groups in the principal cities alone. After the breakdown of the authority of the Egyptian Old Kingdom, for example, there are literary dirges uniformly attesting to widespread destruction, unrest, and impoverishment; yet contemporary provincial cemeteries contained luxuries that were previously monopolized by the capital and court.

In a larger sense then, literary sources oriented toward the interests of narrow social elites probably have permanently obscured the character of the societies in question. It is not merely the rural peasantry but large segments of the urban population about whom we learn only indirectly or not at all. And with regard to the full spectrum of their interests and activities, our information is even more selective. Except for rare, almost anecdotal instances—Thales of Miletus is said to have speculated in olive oil; the great Egyptian vizier Imhotep took pride in his skill as a carpenter and sculptor—little place was given or value

attached to technical knowledge and empirical observation.

The equivalent uncertainties for urban patterns are obvious. The generality of the acceptance of the many unprecedented controls upon social behavior that, to us, are mandatory in an urban setting—the real extent of legitimation of authority, the degree of flux in employment and residence, and the depth of penetration of the great traditions of urban civilization into the lives of both the urban and the rural populace—all are enigmatic everywhere prior to classical times. Except in the Mediterranean basin, they remain so until much later still. The provisional, at times even conjectural, character of what it has been possible to say here about the natural context within which urbanism emerged as a viable and ultimately dominant pattern thus is only an aspect of the prevailing ignorance about our more remote historic antecedents in general.

In spite of this obscurity, it is clear that the conditions we face today are profoundly different ones. A single American farmer produces for 37 of his fellow citizens, not to mention supporting Indian cities halfway around the world with Kansas wheat. High storage dams of reinforced concrete, chemical fertilizers, and agricultural machinery have mitigated the onerous conditions of husbandry as an occupation, multiplied its productivity, and transformed the position of its practitioners in our society. Palaces, cities, literati, the stuff of traditional history, once could be expressions only of the continuing, brutal exploitation of the agricultural masses. They need be so no longer.

Diffidently, with mutual suspicion and at times with sharp conflict, we nonetheless are moving gradually toward an awareness of the new conditions. A continuing reliance on policies of exploitation has become overwhelmingly dangerous. In the long perspective of history, it is perhaps equally important that such policies have become inefficient. Comprehensive programs of economic development, whatever their political coloration, increasingly tend to focus on liberating productive forces and stimulating the widest possible demand. The static opposition of rich and poor over the distribution of a limited surplus accordingly is giving way—at any rate in the developed, metropolitan countries—to progressively wider involvement of the whole society in the process of production and consumption, for only in this way can the growing objectives of any segment be satisfied.

With circumstances so radically different, exploration of the remote past becomes more than ever an academic exercise. Yet if it is ever valid to seek relevance through retrospective generalization, two lessons suggest themselves. The first is that the isolation of urban problems as a separate genre is a legacy of thought from times in which city and countryside were in irreconcilable opposition over the same, pitifully small surpluses with which to sustain life and affirm its human qualities. This is an increasingly unfortunate legacy in a new age when entire nations have become urban "fields." Extended to the international scene, it is reflected in the gap that continues to widen ominously between a handful of metropolitan powers and most of the remainder of the world. We must learn to approach problems of urban life through a succession of widening contexts that override political boundaries and that impose no artificial separations between industry and agriculture, between the needs of cities and those of their supporting areas and hinterlands, and between policies of economic development and policies aimed at extending social integration.

The second lesson flows less from what I have said than from what I have taken for granted. The historic role of cities has been as prime creative centers: seats of learning; sources of artistic and philosophical ferment; initiators and exponents of ecumenical ideas; and forges in which have been fashioned most of our persuasive symbols of, and common aspirations for, a fuller life. This has been so in spite of harsh ecological pressures and consequent repeated interruptions of cultural continuity, and in spite of the thinness of the veneer of even the urban population which formerly could be supported in such "secondary" activities by the subsistence pursuits of others. It would be tragic, as we contemplate the massive urban problems all around us now, to lose sight of cities as the locus of this cumulative achievement. Our task is to save our cities, not to resign ourselves merely to cataloging their defects.

The Cultural Evolution of Civilizations

KENT V. FLANNERY

. . . Human ecology has its moments of enlightenment for prehistory, especially for the study of peoples on a relatively simple and environment-bound level of organization. With the pristine, or any other, civilizations we have moved to what Steward terms a higher level of integration, and additional kinds of causality must be sought. The precipitous ascent from an Early Formative village life to the Olmec civilization is an example of a quantum evolution for which the valid explanation may well lie more in the realm of ideas and institutions rather than in modes of production.

(Coe 1968:65)

In a refreshing way, the new data redress a balance and turn our attention from an overworked interest in the ecology of the ancient Near East back to the more central archeological themes of social organization and cultural content, a subject worthy of more intensive study than it has received in recent years

(Dyson 1967:1420)

INTRODUCTION

During the course of both ancient and modern times, some human societies have evolved to levels of great sociopolitical complexity.[1] The study of these "high cultures," "states," or "civilizations" presents problems of great magnitude, and few attempts to explain them (whether ethnographic or archeological) have met with success. This is no accident; in recent years, a growing body of data suggests that complex societies are simply not amenable to the simple kinds of structural, functional, or "culturological" analyses which anthropologists have traditionally carried out. The limited success of so-called "ecological approaches" to complex societies has led to understandable criticism from humanists, as the quotations which open this paper show. Indeed, there is a widespread belief among both archeologists and ethnologists that ecological approaches are fine for hunters and gatherers and primitive food-producers, but inadequate for the study of civilizations. This is about as convincing as the religious dogma that evolution works fine for all lower forms of life, but Man took an act of special creation.

There is a reason why past "ecological approaches" have failed, and it lies not in ecology but in the self-styled "cultural ecologists." Modern ecologists, who not only analyze but even simulate dynamic ecosystems (cf. Watt, 1966), take into consideration that all populations exchange *matter, energy,* and *information* with their environments. Up until now, it has mainly been the humanists who have studied the informational aspects of complex societies—art, religion, ritual, writing systems, and so on. The "ecologists" have largely contented themselves with studying exchanges of matter and energy—the "techno-environmental" factors as Harris (1971) calls them. To read what the "ecologists" write, one would often think that civilized peoples only ate, excreted, and reproduced; to read what the humanists write, one would think civilizations were above all three, and devoted all their energy to the arts. In this paper I will argue that humanists must cease thinking that ecology "dehumanizes" history, and ecologists must cease to regard art, religion, and ideology as mere "epiphenomena" without causal significance. In an ecosystem approach to the analysis of human societies, everything which transmits information is within the province of ecology. Such an approach will be taken in a later part of this essay.

The first civilizations. The world's most ancient civilizations, including the so-called "pristine states" (Fried 1967), have long been a subject of scholarly

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interest and debate. All evolved before written history began in their respective parts of the world, and all share a striking number of characteristics despite their having arisen totally or partially independent of one another. Thus, although some scholars would argue that the earliest civilizations in the Andes (Peru, Bolivia) and in Mesoamerica (Mexico, Guatemala, Honduras) may have been in at least tenuous contact with each other, overwhelming evidence suggests they arose independently of early civilizations in the Near East, Egypt, and India, all three of which were in contact with each other to an undetermined degree. It is not yet known to what extent early civilization in China was autonomous.

Like other fields, archeology is cursed with terms so vague and ambiguous that they tend to obscure more than they clarify. Since "civilization" is one such term, I will use it only sparingly to refer to *that complex of cultural phenomena which tends to occur with the particular form of socio-political organization known as the state*. The state is somewhat easier to define, since it has been worked over by skilled sociologists and anthropologists. We may lead up to it, however, by briefly considering some of the simpler and antecedent forms of socio-political organization (Fig. 4.1) which have recently been distinguished from states by Service (1962), Sahlins (1968), and Fried (1967).

EGALITARIAN SOCIETY

Bands. Simplest of the egalitarian societies are bands, whose only "segments" are families or groups of related families and whose means of integration are usually limited to familial bonds of kinship and marriage, plus common residence. Leadership is informal and ephemeral; division of labor is along the lines of age and sex; and concepts of territoriality, descent, or lineage are weakly developed. Most important ceremonies are ad hoc, taking place whenever sufficient people are assembled and sufficient resources are available. This organization is frequently found among hunters and gatherers like the Australian aborigines, the Bushmen and Eskimo, and the Paiute and Shoshone of the Great Basin. It is assumed on the basis of archeological evidence that prior to 10,000 B.C. most of the world's population was so organized.

Type of society	Some institutions, in order of appearance	Ethnographic examples	Archaeological examples
STATE	Stratification, Kingship, Codified law, Bureaucracy, Military draft, Taxation	FRANCE ENGLAND INDIA USA	Classic Mesoamerica Sumer Shang China Imperial Rome
CHIEFDOM	Ranked descent groups, Redistributive economy, Hereditary leadership, Elite endogamy, Full time craft specialization	TONGA HAWAII KWAKIUTL NOOTKA NATCHEZ	Gulf Coast Olmec of Mexico (1000 BC) Samarran of Near East (5300 BC) Mississippian of North America (1200 AD)
TRIBE	Unranked descent groups, Pan-tribal sodalities, Calendric ritual	NEW GUINEA HIGHLANDERS SOUTHWEST PUEBLOS SIOUX	Early Formative of Inland Mexico (1500–1000 BC) Pre-pottery Neolithic of Near East (8000–6000 BC)
BAND	Local group autonomy, Egalitarian status, Ephemeral leadership, Ad hoc ritual, Reciprocal economy	KALAHARI BUSHMEN AUSTRALIAN ABORIGINES ESKIMO SHOSHONE	Paleo-Indian and Early Archaic of US and Mexico (10000–6000 BC) Late Paleolithic of Near East (10,000 BC)

FIGURE 4.1. *Types of societies in ascending order of sociopolitical complexity, with ethnographic and archaeological examples of each. A selected number of sociopolitical institutions are shown, in the approximate order in which they are believed to have arisen (see text)*

"Tribes." Although many evolutionists are now unhappy with the term "tribe," Service (1962) originally found it convenient to describe larger egalitarian societies whose segments are groups of families related by common descent or by membership in a variety of kinship-based groups (clans, lineages, descent lines, kindreds, etc), the description of which has kept ethnologists occupied for decades. As Sahlins (1961) has suggested, one latent function of some of these kin groups is as land- or property-holding units, and it is thus not surprising that they are more common among primitive agriculturalists than among hunters. Ancestors are often revered, and it is believed that they continued to take part in the activities of the lineage even after death; good examples can be found among the Pueblo Indians of the Southwestern US (Ortiz 1969) and the New Guinea highlanders (Rappaport 1968). Since "tribes," like bands, have weak and ephemeral leadership, they are further integrated (and even, it has been argued, regulate their environmental and interpersonal relations) by elaborate ceremonies and rituals. Some of these are conducted by formal "sodalities" or "fraternal orders" in which members of many lineages participate; examples include the dance societies, clown societies, or medicine societies of the Pueblo Indians. "Tribes" frequently have ceremonies which are regularly scheduled or "calendric," occurring at the same time every year. These ceremonies—as well as longer-term ritual cycles which stretch out over decades—may help to maintain undegraded environments, limit intergroup raiding, adjust man-land ratios, facilitate trade, redistribute natural resources, and "level" any differences in wealth which threaten society's egalitarian structure (cf. Rappaport 1969:8-9).

Such "tribal" societies seem to have evolved during the early post-Pleistocene period in the various parts of the world we have been considering; they are archeologically manifested in the remains of villages or residential compounds where differences in wealth and status between households are negligible. In the Near East and on the coast of Peru, such settlements appear to have arisen before agriculture and were supported by intensive collection of wild foods; in Mesoamerica, on the other hand, they appeared only after many thousands of years of gradually improving but still-primitive agriculture (Flannery 1972a). The rise of multigenerational descent lines can be seen in some prehistoric Near Eastern villages, where ancestors' skulls were saved and their features reconstructed or where their secondarily-reburied skeletons were stored under the floors of their descendants' houses. In Mesoamerica, multilineage sodalities like the dance societies of the Pueblo Indians are suggested by pottery masks buried with their owners, by countless figurines of dancers in fantastic disguises, and by incredible accumulations of shell rattles, deer scapula rasps, turtle shell drums, conch shell trumpets, and the bones of countless macaws who provided the necessary feathers (Flannery 1972a). Approximate dates for the appearance of egalitarian tribes might be 7000 B.C. in the Near East, 3000 B.C. in Peru, and 1300 B.C. in parts of Mesoamerica.

Chiefdoms

One of the thorniest problems in cultural evolution is the origins of hereditary inequality—the leap to a stage where lineages are "ranked" with regard to each other, and men from birth are of "chiefly" or "commoner" descent, regardless of their own individual capabilities. Since lineages are also property-holding units, it is not surprising to find that in some chiefdoms the best agricultural land or the best fishing localities are "owned" by the highest-ranking lineages. Societies on a chiefdom level include ancient Tonga and Hawaii, the Natchez Indians of the Mississippi Valley, and the Kwakiutl and Nootka Indians of the Pacific Northwest.

"Chiefs" in rank society are not merely of noble birth, but usually divine; they have special relationships with the gods which are denied commoners and which legitimize their right to demand community support and tribute. Frequently, they build up elaborate retinues of followers and assistants (often relatives)—the chiefly precursors of later state bureaucracies. Often, chiefdoms have not only elaborate ritual but even full-time religious specialists; indeed, the chief himself may be a priest as well. Further, the office of "chief" exists apart from the man who occupies it, and on his death the office must be filled by one of equally noble descent; some chiefdoms maintained elaborate genealogies to establish this, and in some cases (e.g. Hawaii) chiefs married full sisters when no one else of sufficiently high status was available. Finally, high-ranking members of chiefdoms reinforce their status with sumptuary goods, some of which archeologists later recover in the form of "art works" in jade, turquoise, alabaster, gold, lapis lazuli, and so on.

Chiefdoms are difficult to identify archeologically, but probably appeared as early as 5500 B.C. in the Near East and 1000-800 B.C. in Mesoamerica and the Andes. One clue used by archeologists is the appearance of burials of infants of high status—status which, because of their youth, must have been ascribed at birth. The child burials with alabaster statues and turquoise and copper ornaments at Tell es-Sawwan in Iraq (5500-5000 B.C.) and those with jade sumptuary goods in basalt-column tombs at La Venta in Mexico (800 B.C.) are frequently cited examples (El-Wailly and Abu es-Soof 1965; Coe 1965:690). Also, chiefdoms have large populations, with the villages of paramount chiefs sometimes running into the thousands, and these may be archeologically detectable. They also have a higher degree of craft specialization, both in necessities and luxury goods. Archeological examples from the Near East include villages which specialized in the manufacture of high-quality pottery, obsidian blades, copper, and flint; in Mesoamerica, there were villages which produced magnetite mirrors, obsidian blades, shell ornaments, or other goods for consumption over wide regions. Yet, although there are village specializations, there is usually as yet no *class* of craft specialists, no occupational castes as in stratified societies. Search every craftsman's house in the archeological remains of a chiefdom, and you will usually find tools which indicate he was a farmer as well.

Stratified Society

States. The next, and highest, form of socio-political organization is the state, and we now come to its definition. The state is a type of very strong, usually

highly centralized government, with a professional ruling class, largely divorced from the bonds of kinship which characterize simpler societies. It is highly stratified and extremely diversified internally, with residential patterns often based on occupational specialization rather than blood or affinal relationship. The state attempts to maintain a monopoly of force, and is characterized by true law; almost any crime may be considered a crime against the state, in which case punishment is meted out by the state according to codified procedures, rather than being the responsibility of the offended party or his kin, as in simpler societies. While individual citizens must forgo violence, the state can wage war; it can also draft soldiers, levy taxes, and exact tribute.

States have a powerful economic structure; they are characterized by both reciprocal and redistributive exchange, and often by markets as well. The economy is largely controlled by an elite (usually hereditary) with preferential access to strategic goods and services; this elite constitutes the usual stratum from which high officers are recruited. As in chiefdoms, the office itself exists apart from the man who fills it; and states have many more offices.

States *usually* have populations numbering *at least* into the hundreds of thousands (and often millions), only a certain percentage of whom are engaged in actual production of food; many are full-time craft specialists residing in urban occupational wards. They attain a high level of artistic and "scientific" achievement, often because of the state's support of, and constant demands upon, artisans of all kinds. States have public buildings, works, and services of various sorts, usually implemented through professional architects, engineers and bureaucrats. Among these will usually appear public works of a religious nature, attended by full-time specialists maintaining a state religion. Such a religion typically has a pantheon of gods with an internal hierarchy and task-differentiation as complex as that of human society itself. In addition, many states use an "official" art style to portray these gods (and the secular rulers who serve them) throughout the area they control or influence, even when those areas are ethnically and linguistically diverse.

The search for "prime movers." What are the mechanisms by which a "tribe" becomes a chiefdom, and a chiefdom a state? This problem has attracted social scientists since Lewis H. Morgan, Friedrich Engels, and V. Gordon Childe. Most recent evolutionary studies by ethnologists are *synchronic*; they take a series of unrelated, contemporary societies on different levels of development and, by comparing them, try to imagine which institutional changes could have turned the simpler into the more complex. Most archeological studies, on the other hand, have been *diachronic*, tracing the development of society through time in a single region. The ethnologists quite rightly point to the richer amounts of detail available in their contemporary societies; yet all their reconstructions amount to "just so" stories, because there is almost no society for which time depth and rigorous proof of evolutionary causes are available. Archeological data lack the richness of detail, yet often provide 10,000 years or more of continuity in a single culture; and many archeologists are now subjecting their data to rigorous testing of a kind that cannot be applied to a synchronic "just-so" story.

Two recent papers by ethnologist Robert Carneiro (1970) and archeologist Henry T. Wright (1970) summarize current theories on the origins of the state. Among the "mechanisms of state formation" which have been proposed are population growth (per se, or in areas circumscribed in various ways), warfare, irrigation, trade, symbiosis between contrasting peoples or environmental zones, "cooperation and competition," and the "integrative power" of religions or great art styles.

Irrigation. Irrigation was originally proposed as a prime mover in the rise of the "hydraulic state" by Karl Wittfogel (1957). He believed that water was a resource of unusual qualities, vital to agriculture in arid lands, yet manipulable by human societies in ways that other environmental variables are not. Wittfogel felt the rise of the state lay in the establishment of a body of rulers and officials who provided the management for large-scale hydraulic agriculture. Carneiro (1970) and Adams (1965; 1966), while granting the importance of irrigation in some regions, reject it as a general mechanism because (*a*) many states, such as the ancient Maya, arose in areas where irrigation is of limited to negligible importance, and (*b*) even in arid Mexico and Mesopotamia, archeological evidence indicates that complex, large-scale irrigation appeared only after the state had already formed (Adams 1965; 1966).

Warfare. For Carneiro (1970:734), "warfare is surely a prime mover in the origin of the state," though "it cannot be the only factor. After all, wars have been fought in many parts of the world where the state never

emerged." With the discovery of possible defensive works, paintings of war scenes, and capture scenes on stone monuments (e.g., stelae at Yaxchilán, Morales, and Bonampak) among the ancient Maya—once regarded as the classic example of a "peaceful" civilization—Carneiro (1970) is probably right in assuming that no early state was without war. But was war really a cause, or a result, of state formation? Most of the evidence cited by Carneiro dates to periods long *after* the state is thought to have formed. In the formative periods which preceded it, the evidence is still ambiguous, and no rigorous test has shown whether warfare results *in* or results *from* the state—or stems from some third factor, responsible for both.

Population growth and social circumscription. Most recently, population growth has been singled out as a prime mover, whose popularity seems almost to have resulted in a new theoretical school. Since the time of Malthus, many social scientists have believed that the adoption of new agricultural technologies led to food surpluses, which in turn stimulated population growth as well as leisure time in which to develop the arts. These views have been challenged (*a*) by Esther Boserup (1965), who suggests that population growth occurs first and provides pressure for new agricultural technology; and (*b*) by Carneiro (1970), Sahlins (1972), and others, who destroy the myths of "surplus" and "leisure time." The cold ethnographic fact is that the people with the most leisure time are the hunters and gatherers, who also have the lowest productivity; even primitive farmers don't produce a surplus unless they are forced to, and thus the challenge is "getting people to work more, or more people to work" (Sahlins 1972). With better technology, people simply work less; what produces surplus is the coercive power of real authority, or the demands of elaborate ritual (see below). This being the case, population growth is now being seen as a *cause* of social evolution rather than a result, in the Near East (Smith and Young 1972), Mesoamerica (Sanders and Price 1968:230), and the Andes (Carneiro 1970:735). A corollary theory has been presented by Carneiro who argues that what is most important is population pressure *within* a *circumscribed area*, e.g., a mountain-ringed valley, or a limited but fertile flood plain. Within such an area, the intolerable struggle for scarce land or resources triggers warfare, which leads to cooperation, competition, mutual defense, and eventually state government to keep peace and allocate resources. And Carneiro stresses that circumscription need not be wholly environmental: peoples living densely packed near the center of an otherwise open area may be "socially circumscribed" by neighbors who surround and impinge on them on all sides, albeit at lower densities.

Complicating the population growth hypothesis is a growing body of data which suggest that human groups (especially hunter-gatherers and primitive farmers) engage in many kinds of behavior which homeostatically maintain their population below the theoretical carrying capacity of their environment (cf. Birdsell 1968). In order for population to grow, people must not simply have more food, they must also cease to engage in such self-limiting practices—infanticide, senilicide, long lactation, ritual sexual abstinence, and so on—to the extent they did formerly. No paper using population growth as a prime mover has yet explained *why* population should grow in the first place, and yet this explanation seems especially incumbent on those who see population growth as a cause, rather than a result, of intensified food production. Moreover, the theory does not go far toward explaining peoples like the Chimbu tribesmen of highland New Guinea (Brookfield and Brown 1963), whose population density reaches 400 persons per square mile, yet who have no kings, no chiefs, no social stratification, no ranking, and indeed, none of the trappings of civilization whatsoever. Among these people, whose exchange is still virtually all reciprocal, environmental and interpersonal relations are regulated not by political power and institutions, but by an incredibly elaborate ritual system which seemingly has evolved as an alternative to power. One can only fall back weakly on the position that population density is relative, and we don't know what density is "enough" to trigger state formation in any given part of the world.

Trade and symbiosis. Several of the areas where early civilizations arose are lacking in raw materials thought to be "essential" to daily life. A lack of building stone, wood, and metal in southern Mesopotamia was long held to be responsible for stimulating trade in that area; and more recently Rathje (1971) has argued that a lack of salt, obsidian, and suitable stone for maize-grinding tools in the Petén region of Guatemala stimulated trade and the rise of lowland Maya civilization. This mechanism, however, does not explain the rise of civilization in central Mexico, which seemingly lacked none of these "essential" raw materials, yet at times had more documented

interregional trade than any other part of Mesoamerica. Moreover, since major settlements are apparently spaced more closely together in the Petén than on its periphery (see discussion of "hypercoherence," below), one could as easily argue that Carneiro's "social circumscription" was operating, rather than resource scarcity. In the Old World, Wright (1969) has demonstrated that in at least one case, on the fringes of southern Mesopotamia, a great leap in volume of trade *followed* the formation of the state, rather than preceding and causing it. Once again, we are faced with a "mechanism" that may have been important in some areas and not in others, thus lacking universality.

Much the same could be said of interregional symbiosis, which is related to trade. Perhaps not surprisingly, symbiosis has been proposed as a mechanism in areas with clear-cut environmental diversity on a "biome" order of magnitude, such as Mexico (Sanders 1956) and greater Mesopotamia (Flannery, 1965), but never in the areas where most of the "civilization" lay within one biome, such as the Nile Valley or the Maya lowlands. Either "symbiosis" needs to be redefined, or it also fails as a universal prime mover.

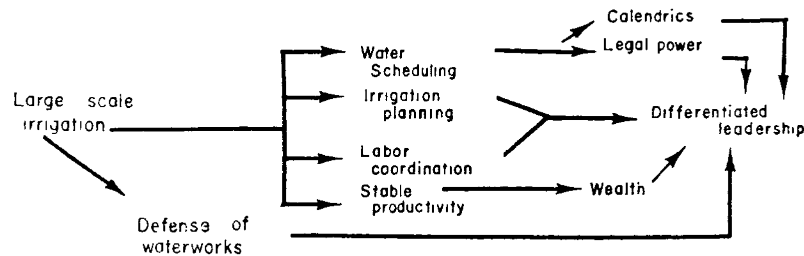
Other "prime movers." We are left with "cooperation and competition" (cf. Sanders and Price 1968) and the "integrative power" of great religions or art styles (cf. Willey 1962), which I will treat only briefly. Though undeniably important, cooperation and competition are generalized processes which go on at all levels of human society, from simplest to most complex; and if they were mechanisms of state formation, there would be no bands or tribes left in the world. In fact, cooperation and competition can as easily function to maintain homeostasis as to promote evolution.

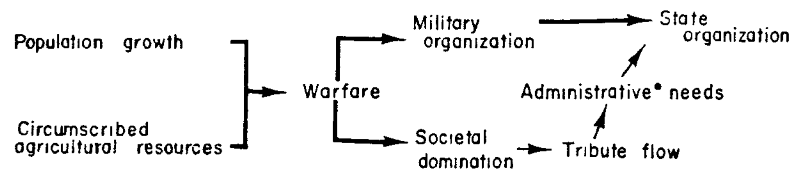
Although the late Stephan de Borhegyi (personal communication) used to say with some justification that "the invention of Heaven and Hell contributed a more powerful prime mover to human society than the wheel," the roles of religion and great art have been handled with imprecision by many anthropologists. It is the hierarchical arrangement of the members and classes of society which provides the actual integration in states. The critical contribution of state religions and state art styles is to legitimize that hierarchy, to confirm the divine affiliation of those at the top by inducing religious experience—the kind of awesome experience that Rappaport, in a previous issue of this Review (1971:31), refers to as "numinous."

Multivariant causality. As Wright (1970) points out, most state origin theories deal either with managerial requirements or with conflicts between social classes or polities. Robert Adams (1966) has produced a theory which is described as "synthetic" by Wright, since it combines both approaches (Fig. 4.2). For Adams there are no "prime movers," but rather a whole series of important variables with complex interrelationships and feedback between them. This model does not satisfy those who, like Carneiro, feel that simple explanations are more elegant than complex ones, but it appeals to those of us who like circular rather than linear causality. In the remainder of this article I will consider some of the implications of multivariant causality.

One way of organizing the variables in such an evolutionary theory is to regard human society as one class of living system, and apply to it a general model for such systems (Miller 1965; Rappaport 1969 and 1971). In such a scheme, the state appears as a very complex system, one whose complexity can be measured in terms of its *segregation* (the amount of internal differentiation and specialization of subsystems) and *centralization* (the degree of linkage between the various subsystems and the highest-order controls in society; see below). An explanation of the rise of the state then centers on the ways in which the processes of increasing segregation and centralization take place. This explanation also requires that we distinguish carefully between (1) such *processes*, (2) *the mechanisms* by which they take place, and (3) the *socio-environmental stresses* which select for those mechanisms. I suggest that the mechanisms and processes are universal, not merely in human society but in the evolution of complex systems in general. The socio-environmental stresses are not necessarily universal, but may be specific to particular regions and societies. It is in this latter category that I place the "prime movers" already discussed, and this categorization helps explain why, although important, they cannot be shown to operate everywhere in the world.

In order to understand how socio-environmental stresses select for certain evolutionary mechanisms, let us diagram a simple human ecosystem (Fig. 4.3a). It consists of a series of subsystems arranged hierarchically, from lowest and most specific to highest and most general. Each subsystem is regulated by a control apparatus whose job is to keep all the variables in the subsystem within appropriate goal ranges—ranges which maintain homeostasis and do not threaten the





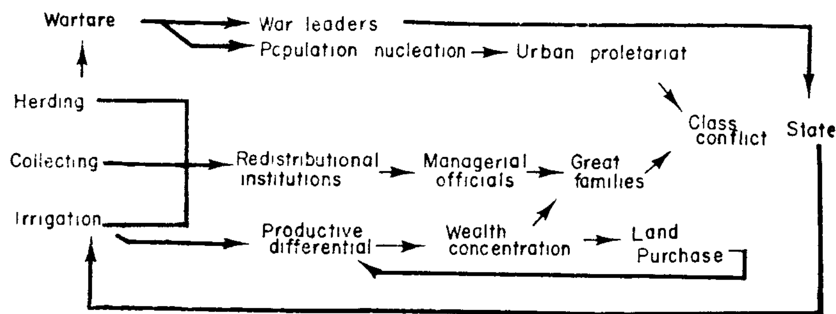


FIGURE 4.2. *Models for the origin of the state proposed by Wittfogel, Carneiro, and Adams. After Wright (1970), with modifications.*

survival of the system. Management of crop plants, for example, might be regulated by a lower-order control issuing specific commands; the distribution of harvests and surpluses (the "output" of the latter subsystem) might in turn be regulated by calendric rituals or group leaders somewhere in the middle levels of the hierarchy. On all levels, the social control apparatus compares output values not merely with subsistence goals but with ideological values, the demands of deities and ancestral spirits, ethical and religious propositions—the human population's "cognized model" of the way the world is put together. The highest, most abstract, and most unchanging of these propositions lie in the highest-order (or "governmental" controls), which deal in policy more often than commands; it is against this abstract set of standards that the human ecosystem's most dramatic events are judged and the need for regulation evaluated. This is almost diametrically opposed to the model used by the "cultural ecologists,"for whom such operations as crop production make up the "core"of culture, while rituals and ancestral spirits are mere epiphenomena (Steward 1955). It also implies that such "epiphenomena"—whose study has fallen largely to the humanist—lie at the heart of society's environmental and interpersonal regulation, and as such cannot be omitted from any comprehensive ecological analysis, as has so often been done in the past.

Normally, higher-order controls regulate only the output of lower-order subsystems, and not the vari-

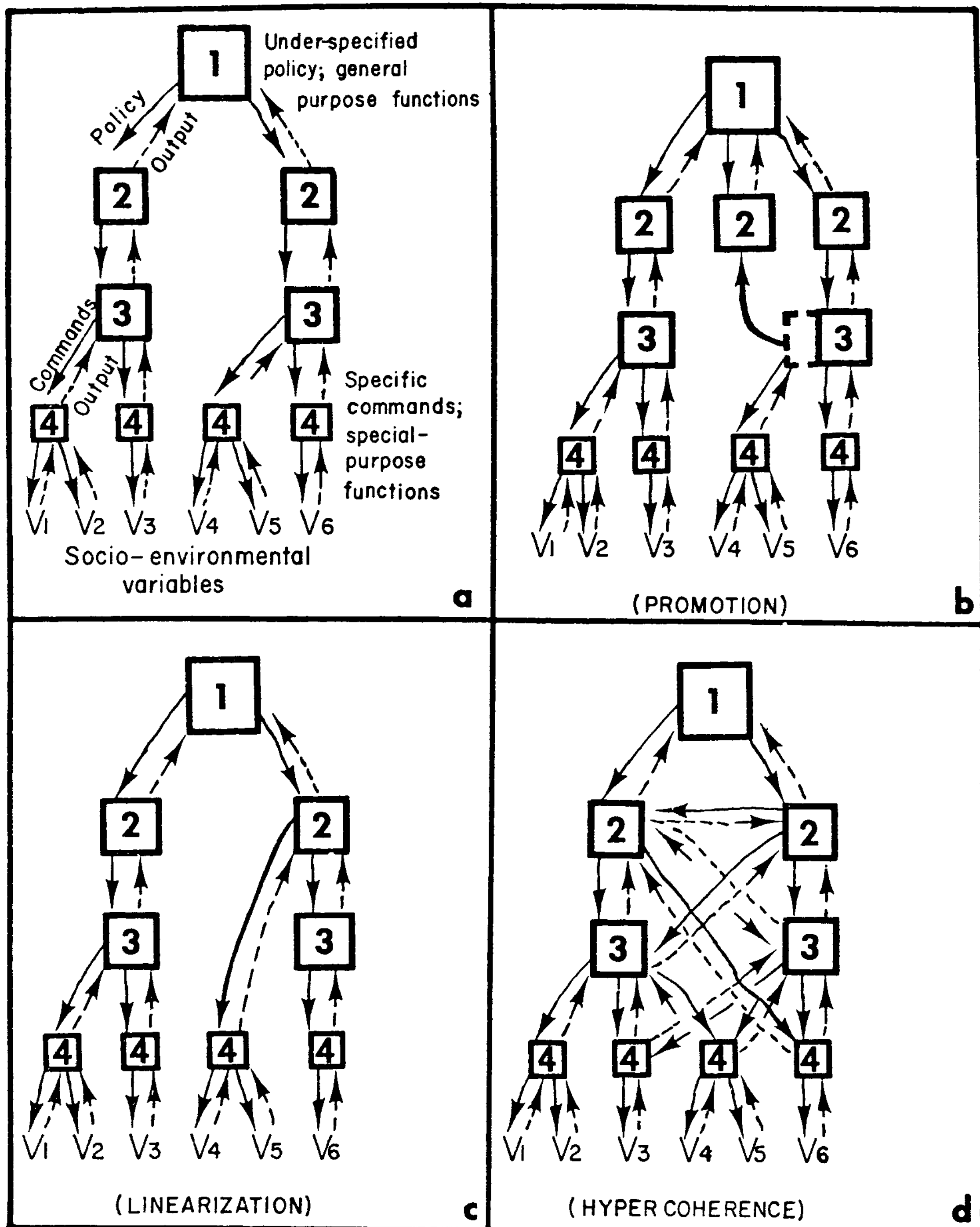


FIGURE 4.3. *Models for the operation of control hierarchies* (a) *the model for a control hierarchy as described in the text, with socioenvironmental variables (V 1-6) at the bottom regulated by low-level institutions (4), and each successively higher level (3-2-1) regulating output of the level below it,* (b) *an example of "promotion" with one function of a third-level institution rising to assume a position of importance in the second level,* (c) *an example of "linearization," with a second-level control bypassing level 3 and directly regulating the output of a fourth-level institution;* (d) *an extreme case of "hypercoherence," with too great a degree of direct coupling among institutions on various levels.*

ables kept in range by the latter. But should a lower-order control fail to keep its relevant variables within their ranges (as in the case of socio-environmental stress), the control apparatus on the next higher level of the hierarchy may be called into operation as a "back up." Should all controls on the levels fail, the system is in trouble; it needs a new regulatory institution, and unless one evolves the system may collapse, or "devolve" to a lower level of integration. If a system is buffered in such a way that deviant variables in one subsystem take a long time to affect other subsystems, it is likely to be stable. But "increasing coherence in more inclusive systems is a concomitant of evolution" (Rappaport 1969:21); this means that more complex, more "highly evolved" systems may be less stable or more demanding, with more direct influence of one subsystem on another, and thus with a need for stronger and more centralized management at the top of the hierarchy. Such centralized, powerful, often unstable systems—of which the state is the ultimate form—are also more successful competitors, in that they quickly push aside or absorb simpler and more stable systems which stand in their way. They seemingly expand continuously until they reach certain scale limits, or until threatened or repulsed by a still more powerful state, such as happened when the Aztec and Inca came in contact with the Spanish.

Much of the control apparatus in human society consists of institutions, which vary greatly in form and function. Especially among lower-order controls, one finds what might be called system-serving or "special-purpose" institutions; these are set up to perform special tasks or regulate certain variables for the good of the system at large. Fewer in number, and found mainly among higher-order controls, are self-serving or "general-purpose" institutions. When their survival is synonymous with the survival of the system at large, they are adaptive and beneficial. When their survival is at the expense of the system, they provide stress. Two of the phenomena of evolutionary advance are the generation of new institutions and their gradual metamorphosis from system-serving to self-serving (see below).

Some of the most important institutions are those which process information for human societies. These are the ultimate detectors of deviant variables; and their numbers increase with more complex societies, for evolving systems—as Maruyama (1963:174) points out—generate new information autonomously through the interaction between their parts. Thus, one of the main trends in the evolution of bands into tribes, chiefdoms, and states must be a gradual increase in capacity for information processing, storage, and analysis.

Among bands, much of the data handling is done by informal headmen, who collect and distribute knowledge about which groves of edible nuts have been thoroughly harvested, which canyons currently have high concentrations of game, and so on (Steward 1955: Chapter 6; Richard B. Lee, personal communication). These headmen support themselves, taking no "overhead" from society; but the number of bits of information they can process is limited, and serves probably no more than 100 persons at any time. With agriculture comes a need to control not only data on wild foods, but the allocation of land, timing of planting and harvest, and collaboration on land clearance; perhaps most importantly, disparities in harvests and surpluses resulting from differential fertility must be regulated for the overall good of the group. Among egalitarian tribes and even in some chiefdoms, elaborate ritual programs detect such disparities and ceremonially circulate harvests, resources, obligations, and rights to land among members of society (Rappaport 1968). These rituals are costly (overhead is still overhead, even when it is ritually committed to gods or deceased ancestors), yet they process more data and perform more regulation than informal headmen.

In chiefdoms—where the number of institutions is far higher, population often very large, warfare frequent, agriculture often complex, crafts more highly developed, and exchange intense—even elaborate ritual may not alone be able to handle regulation adequately; much of it is done by sanctified hereditary chiefs and their retainers, to whom some of the responsibility is delegated. This chiefly retinue is expensive for society to maintain, for it requires sumptuary goods, subsistence, and logistic support for a large group of persons who engage in little or no food production; yet it processes a great deal of data, or processes it faster, and regulates thousands of persons through the establishment of a highly diversified and specialized set of offices on a level above the local headman (Sahlins 1958). In states, the managerial superstructure becomes still more elaborate, multilevel, and centralized; and the royal bureaucracies who process data for hundreds of thousands of souls must be supported by costly tribute, corvee labor, and often

the pillaging of less powerful neighbors (Kottak 1972). In the case of some ancient civilizations, such as the Classic Maya, such a superstructure was supported in spite of agricultural practices believed to be no more sophisticated (except in rare cases) than those of most egalitarian tribes (Dumond 1961). Looked at in this way, the most striking differences between states and simpler societies lie in the realm of decision-making and its hierarchical organization, rather than in matter and energy exchanges. Herein lies another problem faced by those "cultural ecologists" who place such primary emphasis on the ways that civilized peoples get their food.

Socio-environmental stresses and evolutionary mechanisms. Slobodkin (1968) has suggested that when variables exceed their goal ranges, they subject systems to stress which can lead either to breakdown or to evolutionary change. Perhaps as a result of the system's attempt to return a runaway variable to its range, new institutions or new levels appear in the control hierarchy (segregation), or higher-order controls become strengthened (centralization). Warfare, population pressure, the demands of large-scale trade, or any combination of the socio-environmental conditions discussed in the earlier part of this paper may provide the adaptive milieu in which various *evolutionary mechanisms* are triggered. I will discuss only two of these, which I will call *promotion* and *linearization.* They are diagrammed in Fig. 4.3.

In *promotion* (Fig. 4.3b), an institution may rise from its place in the control hierarchy to assume a position in a higher level; it may in the process go from "special-purpose" to more "general-purpose." Alternatively, a new institution may arise out of what was simply one role of a previously existing institution, as the office of chieftainship presumably rose out of the leadership role of the informal headman in a simpler society. Promotion is particularly prevalent among institutions with discrete personnel (Rappaport 1969); it may occur if those personnel are heavily supported or "funded" during a stress emergency, allowing them to become more self-serving when the emergency has passed. Promotion contributes heavily to the process of segregation, since it generates new institutions.

Other examples of promotion may include (1) the evolution of the Sumerian "palace" out of the secular residences included in southern Mesopotamian temple complexes at 3000 B.C. (Adams 1956), with its implications for the evolution of kingship out of some kind of "priest-manager" role in the preceding chiefdom stage; and (2) the transition from the so-called "theocratic" first-generation civilization to their more "militaristic" successors. All the data at our disposal suggest that in the "pristine" first-generation states, rulers were recruited from sanctified royal lineages, while the military was a special-purpose arm of the state. Eventually, however, men of less-than-royal descent, who had risen in the military, seized or acceded to kingship instead of the "proper" royal heirs (Adams 1956). It is possible that unsettled political conditions—times of war and great stress—provided the adaptive milieu in which such special-purpose leaders could promote themselves to general-purpose offices. If so, the failure of normally effective lower-order controls may be indicated. Finally, as a third example, we might hypothesize that the military arose in the first place through promotion of some institution like the "warrior societies" which many chiefdoms have (cf. Gearing 1962).

In *linearization* (Fig. 4.3c), lower-order controls are repeatedly or permanently bypassed by higher-order controls, usually after the former have failed to maintain relevant variables in range for some critical length of time. Examples include (1) the takeover of local irrigation regulation by federal agencies, given below; (2) the bypassing of local headmen by the state when it makes every crime against an individual a crime against the state, outlawing feuds and blood vengeance; and (3) the payment of taxes by each citizen directly to the federal government, instead of the payment of tribute by local chiefs on the basis of the pooled resources of their followers. It must be clear that linearization heavily contributes to centralization. I suspect it is particularly strongly selected for by the need for conflict resolution on all levels of human society, and that it is the mechanism most often triggered by warfare.

Responding to socio-environmental stress, promotion and linearization lead to evolutionary change, but advance is not without problems. Promoted-institutions too often serve their own interests rather than society's, and linearization too often destroys the intervening controls which buffer one subsystem from perturbations in another. Either can lead to what Rappaport (1969) has called a systemic "pathology," which subjects the system to still further stress. In coping with this stress, the system may engage in still more progressive centralization and segregation, and still further evolutionary change; the process is thus one with many positive feedback loops.

Two of Rappaport's pathologies are *usurpation* ["the elevation of the purpose of one's own subsystem to a position of preeminence in a more inclusive system" (Rappaport 1969:26)] and *meddling* ["to subject directly to a higher order control the variables ordinarily regulated by lower order controls" (Rappaport 1969:24)]. As their definitions suggest, these pathologies resemble promotion and linearization, though they need not involve any change in evolutionary level; the instability and further stress they produce, however, can select for one of the evolutionary mechanisms. In a multivariant model, we might see the state evolving through a long process of centralization and segregation, brought about by countless promotions and linearizations, in response not only to stressful socio-environmental conditions but also to stress brought on by internal pathologies.

I will now present three concrete examples of how some of these mechanisms operate. Two, dealing with promotion and linearization, are drawn from the research of myself and my colleagues on the rise of the state in southern Mexico (Flannery *et al.* 1970). The third deals with another of Rappaport's pathologies—*hypercoherence*—which will be defined in another section.

Ritual, promotion, and social stratification. Early speculative historians attributed the origins of social stratification to the "conquest" of one tribe by another, with the victors making slaves of the defeated. Modern ethnologists, however, point out that tribesmen defeated by other egalitarian tribesmen are just as likely to be married or adopted into the victorious tribe. Fried (1967), who stresses the truly central position of this problem, makes it clear that the evolutionary pressures for stratification must be sought *within* society. Elsewhere (1960), he has further suggested that the potential for stratification is already present in egalitarian society, simply waiting for the right socio-environmental context in which to make itself felt. In "tribal" societies, this potential is held in check by what are sometimes called "leveling mechanisms"—social or religious institutions which pick up information on inequalities in landholding, wealth, or power and regulate these variables before they exceed the goal ranges of egalitarian society (see discussion of *mayordomía*, below). In many societies, the accumulation of inordinate amounts of private property by an individual or his kinsmen triggers a ceremony in which he is compelled to give it all away, at the risk of losing face or being accused of witchcraft. In doing so he may gain great prestige, but he does not gain "unequal access to strategic resources or the means of production," which is generally thought to be a criterion of stratification. Nor does he achieve hereditary prestige; his son must gain it in his own way, in his own generation, or not at all.

But close examination of the leveling mechanisms of egalitarian societies reveals an interesting systemic relationship: they often carry within themselves the seeds of their own destruction. Each can, if the adaptive context is right, be manipulated in such a way as to yield hereditary preferential access to strategic resources, in direct opposition to the purposes for which it arose. I will give only one example, from Mendieta y Núñez' study (1960) of San Juan Guelavía, a Zapotec Indian village of traditional maize farmers in the Valley of Oaxaca, 300 miles south of Mexico City.

At the end of the last century San Juan Guelavía was a village of small property holders, governed by a council of elders and regulated by two widespread Mesoamerican regulatory mechanisms called the *mayordomía* and the *cargo* system. The *cargo* is a system of rotation of village governmental offices among the responsible citizens of the town, while the *mayordomía* is a system of rotating financial sponsorship of the fiestas of the town's patron saint and other calendric religious festivals. In principle, the role of *mayordomo* or sponsor will fall over and over again on wealthier citizens who can afford it—thus "leveling" their wealth and distributing its benefits to the rest of the village, while at the same time implicitly legitimizing a tolerable degree of disparity in wealth since sponsors rise in prestige through successful sponsorship of successively more important fiestas (Wolf 1955; Cancian 1965).

In the late 1800s the latent functions of the *mayordomía* were successfully subverted by an enterprising villager named Marcial López (Mendieta y Núñez 1960:216-9), who converted the institution into a means of taking over the lands of his neighbors. With the aid of some friends in the clergy (the special-purpose system in charge of fiestas) he forced the council of elders to designate *mayordomos* without taking into account whether or not the person was sufficiently solvent to undertake sponsorship. Since designation by the council carried a heavy obligation and held out the promise of prestige, the sponsors could hardly refuse even though acceptance forced

them to seek loans; López provided the money, but on condition that they put up their land for collateral. By the end of three decades, López had accumulated considerable property by foreclosure, and by the eve of the Mexican revolution he owned most of the community's best land. Among the Zapotec, debts pass from one generation to another unchanged; a son inherits the debts of his father, and he can end up working his father's former lands as a sharecropper to the son of his father's creditor. By 1915, a few families (mostly Lópezes) owned 92.2 per cent of the arable land in San Juan Guelavía, while the remaining 8.8 per cent was spread among 354 villagers. This preferential access to strategic resources is amplified by the fact that major landowners owned all but 6 per cent of the *irrigated* land. The López family avoided censure by strongly supporting the church (a special-purpose system eager to become a general-purpose system); they had, in one generation, become a "great family" in the sense that Adams (1966) uses the term (see Fig. 4.2). In the end, with low-order controls like the *mayordomía* and cargo "no longer capable of reducing the discrepancy between the deviation signals and reference values" (Rappaport 1969:20), the great families were overthrown and their land redistributed only through "higher-order controls acting as back-ups"—in this case, the Mexican revolution with its policy of land reform.

The implications of this example are several. First, it is evolutionary. It shows the emergence (albeit abortively) of a new institution—the "great family," for want of a better term—and of an economy with preferential access to strategic resources. It is also an example of "promotion," in which a special-purpose institution (the church) took over the selection of *mayordomos*, which formerly had been done by a general-purpose system (the town government, by general consensus). Perhaps most importantly, it shows that evolutionary change can result from the *perversion of a ritual regulatory mechanism*—surely at the farthest remove from the "techno-environmental factors" on which the "cultural ecologists" have generally focused. This is not to say that socio-environmental factors were not operative—they must have been, though in this case we do not know which ones. But their role was to provide the selection pressures, while the actual instrument of change was ritual. And although the results were evolutionary, the mechanism was not unlike Rappaport's pathology—usurpation—in which "regulatory agencies. . . become the instruments of the very subsystems they were meant to regulate" (Rappaport 1969:27).

Linearization, buffering, and the "hydraulic state." A second example from the Valley of Oaxaca illustrates the mechanism of linearization, as well as shedding some light on the Adams-Wittfogel irrigation controversy and the Boserup-Carneiro "population pressure" hypotheses. One phase of our Oaxaca research was an ethnographic study of more than 20 canal-irrigating villages by Susan H. Lees (1970, 1972), from whose monograph the following data are abstracted.

Traditional canal irrigation in the Valley of Oaxaca is a small-scale affair, managed autonomously by each community in its own way. Allocation of water is handled by a variety of methods, almost as great as the variety of towns: sometimes by the municipal president, sometimes by an appointed council, often by the *topiles* (who are little more than village messenger boys). It is simply one of a number of tasks performed by village officeholders, whose positions are rotated among the responsible citizenry by means of the *cargo* system already described. No advantage is achieved through position along the canal system; and even where two villages share the same tributary, Lees found no "depotism" by the upstream village over the downstream village. The construction and maintenance of the small, gravity-flow canal systems is carried out in the ways each community traditionally carries out all other "public" tasks, such as school or church construction, road building, and so on. Moreover, from hundreds of years (or as archeological data indicate, sometimes thousands of years) on the same spot, each village has learned what to expect in the way of fluctuations in rainfall, water table, or stream flow, and can buffer such environmental perturbations.

In recent years, however, the rural villages have become aware of the activities of the Secretaria de Recursos Hidráulicos, a "special-purpose" institution established by the federal government of Mexico to develop the nation's water resources. The SRH has specialists—engineers, designers, hydrologists, construction teams—and heavy earth-moving machinery possessed by no rural village. Several villages near suitable canyons have therefore offered to provide communal unskilled labor if the SRH will build a dam to impound water from their seasonally dry tributary, and a number of dams had already been built when Lees did her study. On completion of such a project—expanding by a considerable order of magnitude the area that can be irrigated—the SRH is

understandably reluctant to leave maintenance of the dam in unskilled local hands. Instead, it leaves the water control of that village in the care of its own appointed representative or *agente*—accountable to the SRH and the federal government rather than the village. Thus the village finds that the price of development is loss of autonomy.

Centralization, therefore, represents a "linearization" of the linkage between the special-purpose arm (SRH) of a higher-order system (the federal government) and an important variable (water) in a lower-order system (the local village ecosystem); response is now direct, rather than buffered by the village government. Moreover, the stage is set for Rappaport's pathology, "meddling." Imagine, for example, a hypothetical case in which flash floods in one or two canyons (a common occurrence in Oaxaca) seriously damage the government-built dams downstream. The news travels directly to the federal government. After several such incidents, a policy decision is made: to prevent future damage due to overtopping, a set quantity of water is to be released each night during the rainy season from all federally built dams. This directive, issued to the entire rural dam system, means that such a quantity of water will be released even by dams in canyons where it has not rained in months. The higher-order system, which cannot possibly know and understand the local environments as well as the individual rural villages do, can thus introduce further instability into the system along with more centralized control. Fortunately, many out-of-the-way villages will simply ignore the directives, thus reducing linearization.

This particular example shows us the trend toward hypercoherence (see below) which centralization can produce. Through linearization and meddling from above, the various canal-irrigating villages in the system are so tightly integrated that disasters in one or two isolated canyons can make their impact felt rapidly on all other villages, through higher-order policies; formerly, a local disaster rarely went further than the local community. Second, the example touches on Boserup's and Carneiro's hypotheses, since in a few cases (but not all!) rural villages *have* called on the federal government for dams because of population growth and land pressure within tightly circumscribed *municipios*. What the Oaxaca example shows is that, while population growth in circumscribed areas *can* exert "causal" pressure for more sophisticated agricultural technology, it is not a direct mechanism of cultural evolution; rather, *it provides a socio-environmental situation in which selection pressures for increasing linearization and meddling are high, and the end result is further centralization.*

Third, we turn to Wittfogel's theory of the hydraulic state. As Lees' study shows, in the case of Oaxaca the state existed (and indeed its corps of engineers and hydrologists *had* to exist) before any "great" irrigation works. But water does possess one unique and critical quality which Wittfogel assigned it: if the federal government wants to meddle in the administration of the rural village, water control is one point at which it can do so. Water is one chink in the armor of the autonomous village—one convenient point at which the government can enter the closed corporate community, perform a service beyond that community's organizational power, and leave the village more tightly coupled to the higher-order system than ever before. Yet it is precisely this linearization—rather than the irrigation itself—which leads to evolution, and linearization can be selected for by a whole range of socio-environmental factors.

Integration, hyperintegration, and devolution. The high level of integration characteristic of states results in part from centuries of linearization, centralization, and promotion. The highly structured nature of even the earliest states often manifests itself archeologically in the absence of written records; in the Near East, for example, Wright (1969) has suggested that the appearance of a three-tiered administrative hierarchy with trimodal site sizes (city, town, and village) may be one indicator of state organization, taken in conjunction with other phenomena. Still another indicator of integration over broad regions is the appearance of the characteristic hexagonal lattices of settlements associated with "central places" on unbounded level plains, developed by Christaller (1933, 1966) and Lösch (1938, 1954).

It had long been known that a hexagon was the most economical geometric form for the equal division of an area between a number of points. From this followed a body of theory, too lengthy to discuss fully here, about the spacing of those towns or cities which act as the centers for distribution of goods and services to smaller towns and the rural hinterland. Assuming (1) uniform distribution of population and purchasing power, (2) uniform terrain and resource distribution, (3) equal transport facility in all directions, and (4) all central places performing the same functions and serving areas of the same size, the most economical

spacing of such service centers would be equidistant, resulting in hexagonal patterns or "lattices." One of the first archeologists to apply this model to an ancient civilization was Gregory Johnson (1972), from whom much of this discussion is taken.

The equidistant, hexagonal spacing of service centers is an important clue which tells the archeologist when the "service functions" of a set of sites—whether economic, administrative, or ceremonial—have begun to strongly override such factors influencing settlement choice as good soil, water, sheltered locale, defense, and so on. Even on the geographer's ideally "unbounded level plain"—a nearly unattainable archeological phenomenon—natural resources are not likely to be evenly distributed. Hence, the settlements of simpler societies are likely to be highly correlated with such resources, and not necessarily regularly spaced. With the evolution of complex societies, "service functions" become increasingly important, and villages which are appropriately located to become "nodes" in the integrated lattice may grow into towns, while their neighbors languish at the village level. Because many important archeological regions are in hilly or rugged country, or linear river valleys, such techniques are hard to apply; and most archeologists applying central place models have deliberately picked the levelest areas they could find.

An example of a hexagonal or rhomboidal site lattice in the Mesopotamian region is provided by Johnson's reworking of Adams' Diyala River survey (Adams 1965; Johnson 1972). Figure 4.4 shows the lattice east of the Early Dynastic city of Eshnunna, with sites of three size classes mapped. The sites

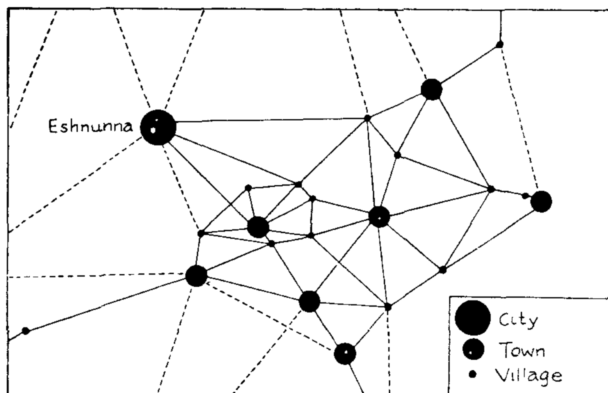


FIGURE 4.4. *A portion of the settlement lattice east of the Early Dynastic Sumerian city of Eshnunna in the Diyala River Basin of Iraq. After Johnson (1972), with modifications.*

forming the Eshnunna "cell" show high correlation (+ .98) with an ideal lattice (Johnson 1972) in spite of deviations due to the alignments of major water courses in the area. Johnson's network can perhaps be contrasted with the region of Uruk in southern Mesopotamia where Adams (1972) feels that central place models are "hardly germane to the hyper-developed urbanism of the late Early Dynastic period" when that city was surrounded only by "a very large number of small towns and villages, unimodally distributed in size rather than forming a differentiated, tiered hierarchy . . . centered on Uruk."

In Mesoamerica as well, some areas seem appropriate to central place models while others do not. Teotihuacán, Mexico's first great city (300-600 A.D.), was so much larger than all contemporary nearby settlements that it fits the anomalous Uruk pattern better than the Diyala lattice (Millon 1967; Parsons 1971). Not only was Teotihuacán on the northeastern margin of the Valley of Mexico, but any "secondary centers" which may have been its contemporaries do not form hexagonal lattices on any significant level. Such urban "megalopoli" were evidently characteristic of some early states.

Though archeologists have typically applauded settlement patterns which show "a high degree of integration," ironically they may in some cases be praising another of Rappaport's pathologies—hyperintegration or "hypercoherence." This highly centralized but sometimes unstable condition results from the breakdown or whatever autonomy the various small subsystems (or institutions) in a larger system may have; one by one, they are coupled more closely to each other and/or to the central hierarchical control until, like an old-fashioned string of Christmas tree lights set in linear sequence, change in one does in fact affect all the others too directly and rapidly (Fig. 4.3d). In Rappaport's words, "it may . . . be suggested that too great a degree of coherence can be as lethal as too little" (Rappaport 1969:20).

One of the most common ways in which hypercoherence can occur is through "meddling" (see above), but there are other ways. Marriage alliances between the ruling families of formerly hostile states, for example, may so strengthen communication and influence between them as to destroy the natural buffering which may have insulated one from the upheavals in another. Such marriage alliances took place with frequency between Sumerian city states (Adams 1966), between Mixtec and Zapotec *caciques*

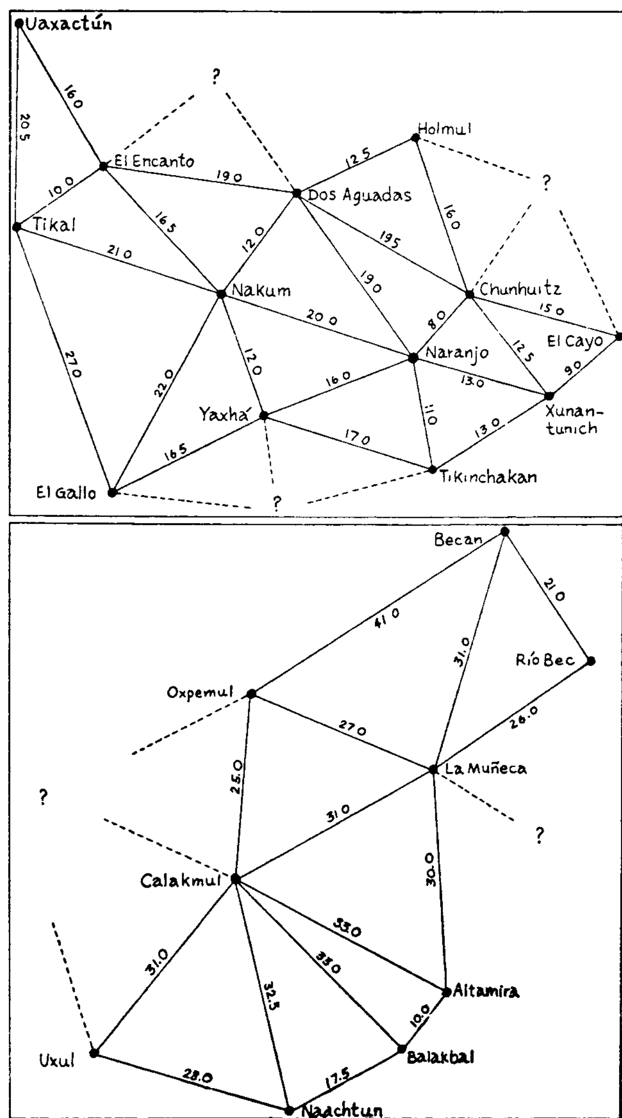


FIGURE 4.5. *Settlement lattices formed by Late Classic lowland Maya ceremonial centres.* Top *sites given in Figure 1 of Bullard (1960).* Bottom: *sites given in Figure 1 of Ruppert and Denison (1943). Distances in kilometers (rounded off to the nearest half-kilometer) are calculated from the original maps.*

in southern Mexico (Spores 1967), and between Classic Maya centers (Marcus in prep.). In another Mesopotamian case, Sargon of Akkad sent his daughter to be high priestess of the goddess Nanna at Ur. Such nepotism certainly increased linearization of the coupling between the main political (Akkad) and religious (Ur) capitals of southern Mesopotamia (Woolley 1965). The critical archeological question is, how much integration is "hyper"?

In this regard, it is perhaps significant that the nearest approach to hexagonal spacing in the settlement patterns of Mesoamerica's lowland Maya civilization occurred in the Late Classic period (600-900 A.D.), shortly before their now-famous "collapse." To demonstrate this, I have used the surveys of Bullard (1960) in the nuclear Petén region, and of Ruppert and Denison (1943) on the northern periphery of the Petén. Only "major ceremonial center ruins" (Bullard 1960) or "sites with stelae" (Ruppert and Denison 1943) were considered. It will be observed from Fig. 4.5 that the hexagons around sites like Naranjo or Calakmul are even more striking than those in the Diyala region, and the lattices are amazingly uniform in view of all the hills, ridges, and wooded swamps (*bajos*) which intervene and distort them. Indeed, if one calculates the distances between "nearest neighbors" among Bullard's major ceremonial centers, one finds the following:

TABLE 1. DISTANCES TO FIRST, SECOND, AND THIRD NEAREST NEIGHBOR AMONG MAJOR CEREMONIAL CENTERS IN THE NORTHERN PETÉN OF GUATEMALA[a,b]

	First nearest neighbor	*Second nearest neighbor*	*Third nearest neighbor*
Mean distance	10.33 km	13.33 km	16.08 km
Variance	3.867 km	4.567 km	3.942 km
Standard deviation	1.966 km	2.137 km	1.985 km

[a]Mapped by Bullard (1960:Fig. 1).
[b]Variance and standard deviation calculated on Constat Program, Computer Service, Univ. Michigan.

Further statistical testing shows that although the difference in means between first, second, and third nearest neighbors is significant (at the 0.0004 level), the difference in variance is not, and the standard deviation from each mean is very low—showing a highly structured situation. This hexagonal lattice, presumably created by the "service center" role of the major sites, is not in itself pathological, but in the Late Classic the centers of each hexagon were linked by marriage alliances and the peripheral centers by military alliances (Marcus in prep.), creating a still higher degree of integration, which may have been "hypercoherent" in our terms. Whatever the cause, the suggested integration is so great that perturbations in one center might have affected other centers strongly, a likely "precondition" for the much-debated Maya collapse (Sabloff 1971). Parenthetically, one might also

conclude from the fact that major sites are almost twice as densely packed (15.8 km. apart) in Bullard's central area when compared with Ruppert and Denison's northern periphery (27.8 km. apart) that Carneiro's "social circumscription" may have been operating. Social circumscription may thus be a powerful stress situation, but in this case it was followed by "devolution" rather than evolution.

Toward a generative model for the state. The ultimate goal of a systems analysis might well be the establishment of a series of rules by which the origins of some complex system could be simulated. Obviously, we are a long way from being able to do this in the case of the state. We do have two evolutionary mechanisms ("promotion" and "linearization"), three pathologies ("meddling," "usurpation," and "hypercoherence"), and two processes ("segregation" and "centralization") which are probably universal. We have half a dozen socio-environmental conditions (population growth, social circumscription, warfare, irrigation, trade, symbiosis), probably none of which is universal, but all of which can select for either evolutionary mechanisms or pathologies and thereby speed the two processes. Let us, therefore, conclude by tentatively putting forth fifteen rules out of the scores with which we might one day be able to simulate the rise of the state.

The process begins in a simple human population with a small set of rules, few institutions, and a small number of subsystems (e.g. a band or egalitarian tribe) forming part of a regional ecosystem. Controls for the lower-order subsystems (e.g. agriculture) are specific and relatively inflexible. Higher-order controls (e.g. "government") are more general and flexible, but set reference values for the output of lower-order systems.

(1) Should lower-order controls fail to maintain certain variables within specified goal ranges, higher-order controls are activated. Repeated activation may lead to "linearization," or "evolution" through centralization.

(2) Linearization weakens the buffers between subsystems, and consequently leads to simplification or lack of subsystem autonomy.

(3) Maintaining such simplification requires more management.

(4) More management requires more formal institutions.

(5) Formal institutions (*a*) may engage in more linearization, thus making rules 2-3-4-5 a "positive feedback" loop, or (*b*) if heavily supported, may be "promoted" to a position in a higher-order system. This may result in the appearance of a new institution, or further "evolution" through segregation.

(6) Evolving living systems generate new information autonomously through the interaction of their parts (Maruyama 1963).

(7) New institutions arise to process this information faster, in larger quantities, or both.

(8) Any institution must grow out of some component of a previously existing institution (often by promotion).

(9) A new institution will appear only after some critical threshold in need for information-processing is reached; thus, evolution appears steplike (cf. Adams 1966:170).

(10) New institutions are initially more efficient, but are also more expensive to support; their "overhead" may provide additional stress.

(11) The evolutionary trend of institutions is from system-serving (special-purpose) to self-serving (general-purpose).

(12) The stress put on systems to support self-serving institutions may require the establishment of yet another special-purpose institution to cope with the stress.

(13) When segregation and centralization reach a certain threshold, the state can be said to exist.

(14) Enough centralization, promotion, and linearization may move the state toward hypercoherence and instability.

(15) Finally, hypercoherence can lead to collapse and devolution.

Obviously, these few simple rules are only a tiny first step toward understanding the cultural evolution of civilizations. Such multivariant models, though many find their complexity repellent, can have certain beneficial effects. First of all, they force the investigator to be specific about the linkages between variables, thus distinguishing between socio-environmental selection pressures (which are local) and mechanisms and processes (which are universal). Second, they emphasize the importance of information and ritual in the regulation of environmental and economic variables in human society. They may thus provide the meeting ground for both humanist and ecologist. For Coe and Dyson, in the quotations which open this paper, are partly right and partly wrong. They are right when

they say that ideas, institutions, social organization, and cultural content have been grossly ignored by ecologists interested in the rise of civilization. What is wrong is their implication that those topics fall somewhere outside the province of the ecologist. Particularly for ecologists interested in the state, they are even more important than the ways such complex societies produced their food.

ACKNOWLEDGMENTS

I acknowledge with thanks the constructive criticism of Richard Ford, Conrad Kottak, Susan Lees, Roy Rappaport, Andrew P. Vayda, and Henry Wright None agrees wholly with what I have said, nor should they be held responsible for any factual errors or fuzzy thinking. I also benefited from the discussions at a recent conference, "The Socioeconomic Aspects of Ecological Stability and Change," sponsored by the Social Science Research Council

NOTE

[1]The reader should note that complexity of sociopolitical organization does not imply ecosystem complexity. Indeed, as this article will suggest, some of the most complex societies may be associated with deliberately simplified ecosystems. For example, the intensive monocrop cultivation which supports some states is ecologically less complex than the diversified and eclectic wild-plant harvests of some hunting and gathering bands.

Part II The Nature of the Indus Civilization

Editor's Introduction

As an introduction to more specialized topics three papers have been selected which deal with the Indus Civilization in a wholistic way. Each has its individual focus, yet if read together they cover a range of issues and problems which anticipate more detailed discussions which are to follow. Sir Mortimer Wheeler's third edition of *The Indus Civilization* (1968) remains an essential source for everyone, but Walter Fairservis and M.R. Mughal have their own thoughts and insights which at times are different from his. Given the immense impact of Wheeler's writing on these matters, these alternative propositions need a serious exposure, if only to introduce variety in thought and approach.

The first two papers by Fairservis represent the most encompassing, anthropologically oriented statement available to us today. Decidedly ecological in his orientation, he begins with a close evaluation of the environmental variables attendant upon life in the greater Indus Valley and Baluchistan. He argues convincingly for what he suspects is climatic stability and ecological continuity within this zone for the past four thousand years. This view is shared by R.L. Raikes and R.H. Dyson (1961) but has been recently challenged by G. Singh (1971) and C. Ramaswamy (1968) (see Part VII). Singh's work in particular is very promising, and may ultimately prove to be correct. However, the interpretation of pollen sequences such as those he recovered from several salt lakes in Rajasthan is extremely difficult and his chronology lacks strength. With this qualification the best working hypothesis remains the one which suggests basic climatic equilibrium. One of Fairservis' major conclusions—that the Indus Civilization expanded to the limits of its ecological zone—is to some extent dependent on the resolution of this important issue, but nonetheless deserves to be explored more completely. For example, our understanding of the important "Kulli Culture" of Baluchistan (Piggott 1950:95-120) within the general context of urbanization in western South Asia depends to a large degree upon an assessment of the cultural ecology of the region as a whole. Fairservis' conclusion will also play a role in arguments for continuity and discontinuity within the culture-historical record of the larger Indus Tradition beginning with the earliest village farming communities in the fourth or fifth millennium B.C. and continuing through the the Post-urban Phase of the civilization in the late second millennium.

Beginning with the fact that Harappan subsistence was centered on the West Asian constellation of domesticated plants and animals (wheat, barley, sheep, goats and cattle) Fairservis develops a theme. Within the Early and Mature Harappan, that is the Pre-urban and Urban Phases, there is a close agreement between settlement patterns and the geography within which these same plants and animals have historically formed the subsistence base. During the Post-urban Phase of the civilization this coincidence of geography and subsistence seems to change. During this time in Gujarat millets become the predominant food grain. The settlements associated with Ochre Colored Pottery in the western Ganges Valley fall within the transitional zone where wheat cultivation gives over to rice. This would seem to indicate that the Harappan subsistence system changed in some significant ways in the time following the abandonment of the cities of Mohenjo daro and Harappa. As noted elsewhere (Possehl 1974:143-168) the rather *significant growth in the numbers* of village farming communities in Gujarat within the Post-urban Phase is hardly indicative of an end to the civilization. In fact, this Post-urban Phase marks the high point of settled,

village based, agricultural communities in the region. As Fairservis suggests the interplay of culture and ecology has much to tell us in understanding these processes.

In a somewhat more speculative way Fairservis attempts to deal with Harappan settlement patterns. He begins with a description of location and relationship between city and village in the Indus Valley and then compares it to what we know of Sumer. The contrasts which he finds here are provocative. The settlement organizations of these two civilizations are strikingly dissimilar. In ancient Sind and the Punjab, the pattern was far more "open" than that in Sumer, with relatively large distances between the settlements, especially the cities. This observation leads Fairservis to the conclusion that: "Accordingly, we can assume that the life of the Indus farmers centered on the villages and not the town or city and, under such conditions, the dispersal of fields was at a maximum" (see page 57 below). To be sure the data from which he is forced to generalize are thin indeed. Only one reconnaissance has been completed near Mohenjo daro (Majumdar 1934) and surprisingly, there has been essentially no exploration in the vicinity of Harappa. Still, Fairservis offers us an extremely interesting, and testable hypothesis. Systematic survey can confirm or deny this proposition. It is not a question of the settlements having been lost under the layers of alluvium, for we know that in Mesopotamia, an environment comparable in this regard to the central Indus Valley, very early sites can be detected on the surface (Adams and Nissen 1972). If there were large numbers of ancient villages in the greater Indus Valley they can be found through conventional exploratory techniques.

In Fairservis' second paper his ecological paradigm is developed into a sophisticated, anthropologically oriented, developmental scheme and demographic statement concerning the civilization. The latter is particularly significant since it is nowhere paralleled in other literature on the Indus Civilization. For example, his population estimates for various sites, including Mohenjo daro and Harappa, are to date the most comprehensive and rational. The developmental model addresses a much more thoroughly debated issue in Harappan studies and ought to be read in conjunction with M.R. Mughal's *The Present State of Research on the Indus Valley Civilization.*

Mughal's paper is an evenhanded attempt to assess the critical problems facing those interested in the Indus Civilization. He successfully covers a great many themes—sea trade, settlement patterns, chronology, the so far undeciphered Indus script, and the eclipse of the urban centers—in a commendably concise fashion. Of particular interest is his view of the development of urbanization in the lowlands and his isolation of an Early Harappan cultural horizon. Essentially, he provides the reader with a summary of the ideas he first expressed in his dissertation *The Early Harappan in the Greater Indus Valley and Baluchistan* (1970). It will be evident that Mughal's views complement those of Fairservis and if interdigitated, provide us with the best current model for the growth of urbanization in western South Asia.

The Harappan Civilization: New Evidence and More Theory

WALTER A. FAIRSERVIS, Jr.

The Harappan civilization unquestionably ranks as one of the great civilizations of the early ancient world. Its importance stems largely from the fact that it represents the first of the successful achievements of civilization beyond the bounds of the lands that constitute the Fertile Crescent. Clearly, its cultural contributions not only to India but to world civilization were far from inconsequential. Its significance and uniqueness have been lucidly described in recent years by a group of British archeologists, i.e., D.H. Gordon, Stuart Piggott, V.G. Childe, and Sir Mortimer Wheeler. The last has been, in fact, not only a pioneer in reevaluating the old data but in presenting new evidence bearing upon the origin, character, and collapse of the Indus River civilization. His recent detailed study (1968) and Chapter 5 of his 1959 work, an essay, are invaluable contributions.

After reading Wheeler (1959), one concludes that interpretations of the Indus civilization have changed very little since Piggott's study published in 1950. More recent research has expanded its geographic dimension and qualified some of Piggott's conclusions, but, in general, one is impressed with the absence of any really significant addition to the body of information on the subject in the last decade. Scholars believe the Harappan civilization possesses the following features:

The Indus civilization began "explosively" as the result of successful colonization of the Indus Valley by Harappans who won out after centuries of "failure succeeding failure" by earlier "pioneers." Once established, the Indus people achieved the "lightning subjugation of the large valley and adjacent coast." The civilization centered around two metropolitan centers, but stretched out from western Baluchistan to beyond Delhi (Alamgirpur) and from Punjab to the Narbada, forming the "most extensive civilization of the preclassical world."

An authoritarian regime, with priestly attributes which compelled cultural uniformity, probably controlled the civilization. The implication is that the rulers controlled from dual capitals in which were situated "embattled" acropolises or citadels, centers of both ceremonial and secular functions. If this interpretation of the data is correct, then the Indus civilization exemplifies "the vastest political experiment before the advent of the Roman Empire."

Though there are outstanding differences there are also general affinities to early Mesopotamia whence the Indus people had received the "idea of civilization"; in some instances, perhaps more than an idea, for "as a scatter of Indus seals and other trifles in the Mesopotamian cities show, it traded with its neighbors of the Persian Gulf from the 800 miles of coast line which we must now allot to it."

The character of the culture is generally static, the result of a probable "complacency, even a self-satisfaction, which impeded further effort." As Piggott described it: "The secrecy of those blank brick walls, the unadorned architecture of even the citadel buildings, the monotonous regularity of the streets, the stifling weight of dead tradition all combine to make the Harappa civilization one of the least attractive phases of ancient Oriental history."

The Harappan civilization lasted perhaps one thousand years, varying in its content as little through time as through space. However, indications of decay in the late phases, especially in Sind, can be ascribed partly to man-caused deforestation which "checked the transpiration of moisture and reduced the rainfall." A more

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abundant rainfall and denser forests in the occupied area in earlier times are implicit. From the fauna, the quantity of fuel necessary for firing millions of bricks, the agricultural activity required to support the large cities, and the existence of drains, Piggott concluded that the climate of Sind was more moist when Mohenjo daro flourished than in modern times. Wheeler appears to concur in this conclusion. Thus "wearing out" of the landscape was a major factor that contributed to the eventual collapse of the civilization. The other major factor may well have been the militant forces from the west; among whom the Aryans are good candidates for the destroyers of Mohenjo daro.

Admitting that summaries like the preceding are unsatisfactory in that they may reduce a profound and scholarly treatise to bare bones, I think that the abstract is a fair presentation of the current view held by colleagues and the general public as a result of the Wheeler-Piggott thesis. Certainly, it is repeatedly cited in American texts.

Objective re-analysis of the available evidence, in conjunction with new fragments derived from recent archeological investigation, indicates that there are possible alternative interpretations and, in fact, some disagreements that lead to a view of the Harappan civilization that differs from the one presently held. At this stage of our researches it seems worth while to present these alternatives in the hope that they will stimulate new investigations and suggest new ideas which will supplement and enhance the pioneer work of British scholarship. I question parts of the interpretation summarized above on several general grounds.

Climatic change in both Baluchistan and Sind is a vital subject, bearing as it does upon the environmental pressures on the cultures concerned. It is significant that only in those areas directly affected by the waters of the Indus River does the flora differ somewhat from that found in Baluchistan.[1] The delta of the Indus consists of mangrove marshes, while forested land composed of babul, tamarisk, kandi, sissu, and bahan occurs close to the banks of the river itself. However, these trees also occur in Baluch forest areas such as northern Las Bela, Kolwa, and Makran. In general the difference between Sind and Baluchistan is quantitative rather than qualitative.

Grasslands are, according to Pithawalla (1959), "now largely occupied by the area under irrigation and cultivation." One must note also the extensive, seasonally fluctuating marsh areas around Lake Manchhar. The critical fact about these forests is that they are riverine, that is, they depend upon the flood water of the river for their "sustenance and growth" (Pithawalla 1959), not upon rainfall. Further, one must remember that the biotic region of which Sind is a part includes Punjab, Rajasthan as far as the Jumna, Cutch, and most of Gujarat; in other words, precisely that area in which the Harappan civilization flourished. Significantly, this region includes the bulk of the major grain-growing[2] areas of the Indian subcontinent (Spate 1957:Fig. 49).

The fauna associated with the Harappan civilization is, without exception, dependent on grassland and open forest country. In fact, in the case of the bear, tiger, and sambar deer, the open hills of steppe or scrub forest type, such as those of the Kohistan of Sind or the Bugti Hills, are probably perfectly suitable. The rhinoceros, on the other hand, as well as the elephant and the buffalo, prefers high grass. Naturalists tell me that had there been a more rainy climate in the past, we would expect to find intermittent pockets of survivals of the earlier ecology. Under those circumstances, the wild life involved would include the smaller mammals, such as shrews, moles, mice, and the like, as well as reptiles and amphibians. Such faunal pockets occur nowhere in the Indus Valley nor are pockets reported for the flora.

Unquestionably, man was the chief exterminator. The fact that the grasslands were the habitat of the big game and, coincidentally, the areas best suited for agriculture and the grazing of domesticated animals spelled the doom of the larger wild life. Spate (1957) points out that during the sixteenth and seventeenth centuries "the Moghul Emperor hunted wild elephants and buffaloes, bison, rhinoceros, lions, and tigers in the Ganges-Jumna doab." Most of these animals are now completely absent in that area as the result of man's actions.

Thus the evidence points to a similarity of climate from ancient to modern times, so that we can conclude that changes in the natural ecology were the result of man's activities.

But there is other evidence. In Baluchistan it is a remarkable but nevertheless definite fact that the bulk of the known pertinent prehistoric sites are located in close proximity to modern villages, indicating a similar dependence upon identical water and soil resources. In the Indus Valley there is clearly a similar situation, for the larger percentage of Harappan sites are in the midst or at the edge of modern cultivation, the exceptions

being those of the Ghaggar River for which there is another explanation.

The three principal forest trees, the babul (*Acacia arabica*), the tamarisk (*Tamarix gallica* and *dioica*), and the kandi (*Prosopic spicigera*), as well as a group of minor trees, are the sources of local fuel today, just as they have been for centuries. It is of interest that in 1959 the new museum and rest house at Mohenjo daro were being constructed of brick made from local clays and fired harder than Harappan bricks by kandi wood fuel. This wood apparently grows abundantly and rapidly and produces a hot fire at a relatively low rate of fuel usage. Until we know more about Harappan kilns and the necessary amount of fuel for burning bricks of the hardness required by Harappans, it would appear to be inconclusive to use the quantity of bricks as a criterion for determining more extensive forests.

Similarly, drains, although their presence may be considered as a part of a ritual complex (Fairservis 1958: 508; 1959: 308), must also be viewed as essential to a sewage system. The latter is certainly a part of the Harappan contribution to the world. Again, we should note that many drain channels at Mohenjo daro are covered, which seems impractical if they were constructed primarily to carry off rainfall.

The need to fire brick in order to preserve structures built in moist environments possibly had its source in the necessity to resist flooding, apparently a continuous problem that was never really solved, as the mortar in any case was apparently soluble.

Lastly, I must consider the problem of the gobarbands or dams discovered by Stein and others in Baluchistan. In a forthcoming study of sites in Las Bela, I present evidence to prove that a dam discovered near an Amri site on the Upper Hab River was built to catch the meager annual run-off from the surrounding mountains and by storing it to render it available to normally arid silt tracts which the position of the site indicates were cultivated (Fig. 5.1). The presence of bund agriculture on the daman in southwest Sind (Spate 1957: Fig. 459) and the use of an identical bund by Arabs, and earlier by the residents of a Harappan village on the edge of the Malir oasis, would indicate that these dams were constructed as a rather desperate attempt to store the available moisture. Such an attempt recalls the measures of the Nabateans in the Negeb (Evenari and Koller 1956).

We are urged by the evidence to these conclusions:

(1) In all probability there has been no significant climatic change in the Indo-Iranian borderlands during the past 6000 or more years, a point that permits us to assess the ancient situation on ecological grounds familiar today.

(2) There appears to have been ecological continuity between the Indus Valley and Baluchistan, the differences being a matter of biotic density brought about by a difference in quantity of moisture resources.

(3) The Harappan civilization spread to the limits of the original ecological zone.

The second major basis of alternative theory can be simply stated as anthropology versus history. Braidwood (1957:74) has pointed out certain basic differences in the orientation of some British and American archeologists. Such differences in orientation cannot help but affect interpretations, which is not to say that either one or the other is wrong or right, but that there is a clear difference in emphasis, and this in turn can cause a difference in interpreting the processes involved in the creation and sustaining of a civilization or culture.

Thus in the historical approach we have dynamic processes instigated by individuals who establish, colonize, and rule. More frequently than not collapse occurs as the result of invasion, and the chronological charts are marked by "destruction" levels. This historical orientation, or emphasis upon event, directly affects one's views and even one's excavation technique (Fairservis 1956a:202-203). In contrast to this emphasis we take into consideration the anthropologist's awareness of culture as a dynamic phenomenon composed of traits continually changing through time and space. In other words, the anthropologist is aware of the evolutionary character of human cultures and factors such as diffusion and acculturation which influence the rate and the quality of this evolution. The present discussion of the origin and character of the Harappan civilization provides a good illustration of these differences in emphasis. Basically, of course, these approaches are not at opposite poles, because archeological interpretation tends to use elements of each.

Both V. Gordon Childe and Stuart Piggott used the term "peasant" to qualify the numerous cultures of Baluchistan. This term attains meaning when contrasted with "urban" or "imperial," labels frequently used by these authorities for the Harappan civilization. In point of fact, as has been shown in another context, the bulk of the Baluch prehistoric cultures had already vanished by the time the Harappan of Sind was flourishing (Fairservis 1956b). The data on the Baluch



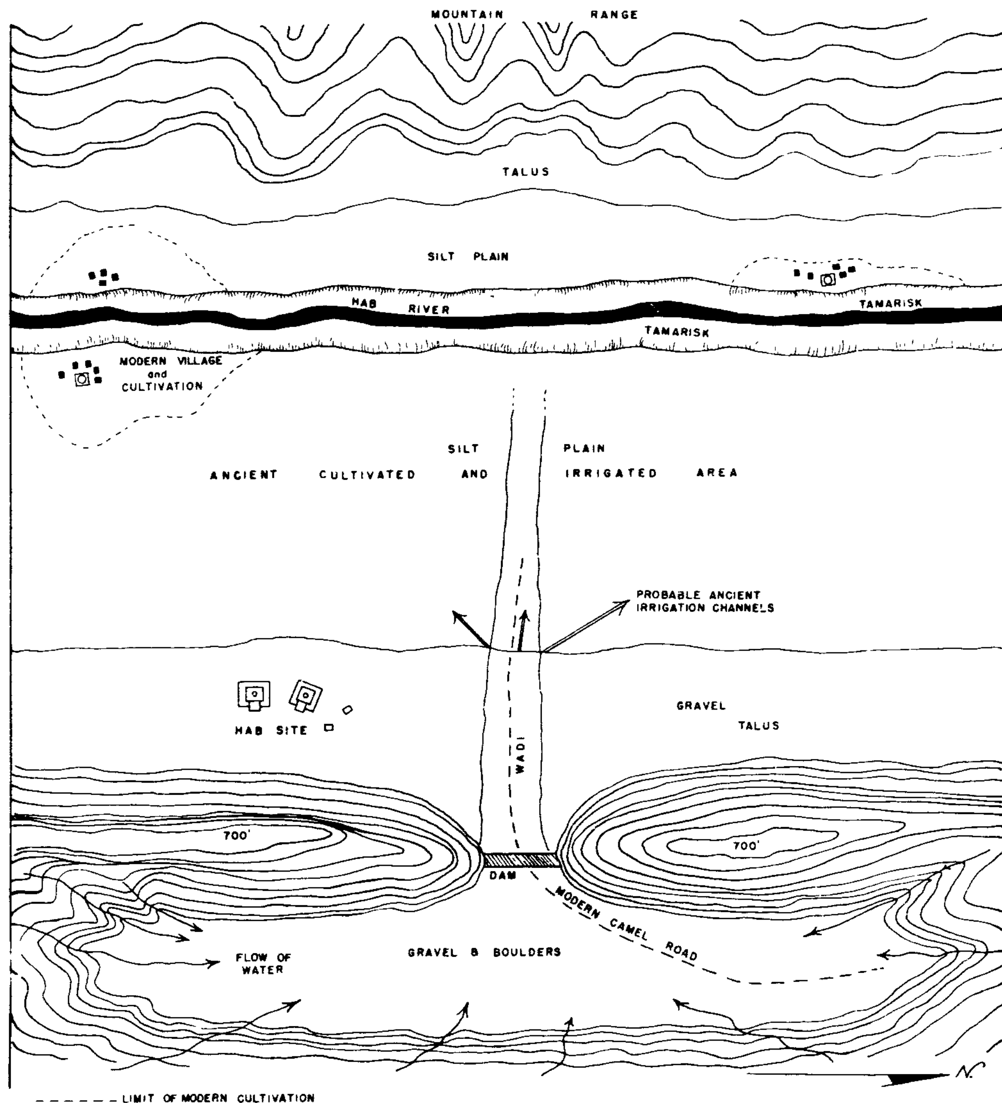


FIGURE 5.1. *Sketch map of the site of the prehistoric dam near Diwana on the upper Hab River.*

cultures indicate that the Harappan culture was merely the last and probably the most elaborate of a long series of cultural phases. In the Quetta Valley, for example, a continuing sequence based on archeological excavations revealed traces of phases of development beginning with villages dependent on limited agriculture and sheep and goat husbandry and expanding to an elaborate ceremonial complex, complete with monumental buildings, "fertility" figures, and the probable use of ablution and sacrifice as a part of ritual (Fairservis 1958, 1959; Alcock in Fairservis 1956a). It is clear that this final phase in the Quetta Valley has its equivalents in Loralai, Zhob, Kalat, and Las Bela.[3] In the last-named area the number of sites is so great and they are so closely adjacent to one another as to approach urbanization.

The cultural influences discernible in this sequence indicate that its earlier phases are Iranian. Later, however, an apparent "Indianization" took place, formalizing the specific character of the later Baluch cultures and setting them off from those of the remainder of the Iranian plateau. This Indianization includes such obvious features as figurine decor, elements of ceramic design, the construction of drains, and very probably the dominance of the raising of cattle (*Bos indicus*) over that of other animals. The

probability is that this Indianization took place because of the rise of equivalent but more Indianized cultures in the neighboring Indus Valley with which the Baluch cultures were in some contact.

It is now apparently an assured and accepted fact that the Near East was the nuclear center for the beginnings of agriculture, domestication, and metallurgy, and that farming communities developed outside this nuclear center as a result either of direct migration or stimulus diffusion or both. The spread of village cultures eastward across the Iranian plateau is amply indicated by the numerous sites found there wherever patches of soil and trickles of water permitted a modicum of cultivation. In Baluchistan one finds places such as Kolwa, Welpat tahsil in Las Bela, the Quetta Valley, and the area around Duki in Loralai, where there are large concentrations of sites, presumably because the quantity of good soil and water exceeds that found elsewhere in Baluchistan. In most cases it appears that this concentration of sites began earlier than the Harappan of Sind. Again in western Sind, especially around Lake Manchhar, Majumdar (1934) has demonstrated that there existed a flourishing pre-Harappan settlement which he labeled Amri. The Amri cultures are found in Baluchistan and also have strong ties in the Nal, Kechi Beg, and Sur Jangal phases and can be regarded as much a part of Baluch cultural history as that of Sind. This point is important for it indicates that we have confused the problem by drawing a rather rigid boundary between Baluchistan and Sind, when from both the environmental and cultural point of view such a boundary is non-existent. In effect, we can say that the only important difference between the flourishing areas mentioned above and Sind is a quantitative one: there are more arable or cultivable soil and water in the Indus Valley than anywhere in Baluchistan or on the Iranian plateau. It is possible, then, that Sind must have offered a lodestone to early farmers, not a barrier. The flow of men and their cultures from west to east would naturally have moved into the Indus Valley with few obstacles. I must confess that I cannot agree with the inference behind Wheeler's description of that valley as coincidentally a place favorable to man and also so "menacing" that early man hesitated to occupy it. Certainly, compared to the situation in Baluchistan, the Indus Valley, with its ampler water supply and game, and rich plant growth, was more attractive. Whatever the problems of salinity, flood, and fever, these would appear to be minor as compared to the ever-present possibility of moisture failure so prevalent in Baluchistan. It seems to me that from the very beginning of agriculture in the Indo-Iranian borderlands people must have found Sind attractive. Accordingly, I suspect that, as our researches are intensified in Sind, we will encounter developmental phases equivalent to those found in Baluchistan. Already hints of such a development are offered by the discoveries at Kot Diji, and the demonstration of at least two phases of the Amri "culture." Recent excavations at Utnoor in Andhra Pradesh have revealed a cattle-using settlement, surrounded by stockades and possessing limited pottery. This settlement is dated to about 2000 B.C. or contemporary with Early Harappan on a conservative chronology (Ghosh 1959d:11). If this date is established, this discovery may indicate a considerable precedent of cultural diffusion across the Indus Valley down into the Deccan prior to the rise of the Harappan civilization and so help to confirm the idea of a long pre-Harappan occupation in the valley.

The abundant finds of artifacts of "Mesolithic" type in various places in northern and western India suggest that peoples with hunting cultures lived in and about the river valleys where game was abundant. Again, as has been suggested in another context, we can assume that a variety of "forest" hunting cultures existed in the Indus Valley as elsewhere (Fairservis 1952b). It is of course impossible to judge what contributions such forest cultures may have made to the agriculturists, but we might at least assume the beginning of Indianization. The earliest agriculturists there may have established characteristic Indian features.

It appears that the subcontinent was never really subjugated to a cultural vacuum. This fact, as far as I can see now, is the basic reason why new cultural elements were retarded in their movement from the Iranian plateau. A rather dramatic example of this retarding phenomenon appears on the Upper Hab River Valley where, as I indicate above, a site of late Amri time is found close to the dam that supplied the moisture for its crops. Here the high alluvial silt plain slopes towards the center of the valley. The modern villagers cultivate close to the banks of the Hab River which flows between high bluffs. The bluffs retard the use of the river water to irrigate the silt plain, compelling the local villagers to till only the small areas close to the banks where there is sufficient water to supply their needs. In contrast, the Amri site is situated at the edge of the arid silt plain close to the valley wall

(Fig. 5.1). Examination of that wall reveals a gap in the front range through which torrents pour following rainfall. Behind the front range is an extensive catch basin which concentrates the water. The major outlet is through the break in the front range. The Amri people sealed the gap with a stone block dam 25 feet thick, thereby forming a reservoir. Apparently the impounded water was released to irrigation channels that ran across the silt plain, thus permitting its cultivation. In an area that probably never had more than 10 to 12 inches of rainfall annually, the control of the water was vital and undeniably required the centralized energies of the entire community. It is of interest that the surface remains of the site, primarily stone, consist of thick walls, paved floors, stairs, and what appear to be drains. The site may represent a control center for the irrigation system and as such may have had particular significance. Small villages were probably spread out over the plain, as the site itself appears too small to have contained the population necessary to build and maintain the dam and to cultivate the large irrigated area. The effort involved must have been very great. The site is only about 35 miles from Lake Manchhar across easily traversable passes. Certainly it could not have been an environmental barrier that forced this population concentration in an arid valley. I submit that the Indus Valley was at that time already settled by peoples who were just as knowledgeable as those of Baluchistan. However, their presence in the Indus area slowed population movements into Sind which, in turn, caused population concentrations in eastern Baluchistan to the limit of its productivity. The numerous Amri sites found in western Sind may well explain the cultural barrier in Sind in pre-Harappan times.[4]

At Kot Diji the fortification found below the Harappan levels indicates a centralization comparable, perhaps, to that of the Amri culture. Our evidence then suggests not only a considerable occupation of the Indus Valley in pre-Harappan times, but one that was approaching if not achieving urbanity. A number of typically Harappan features, including figurines, bangles, and decorative elements, have been found in pre-Harappan context at Kot Diji. Most of the designs common to those of the Harappan painted wares occur within the Amri culture just as they do in Baluchistan. In fact, a comparison of the drawing techniques suggests that the Harappan pottery with its overall patterning and generally irregular slap-dash designs had already arrived at a declining stage of pottery painting. The thickness of Harappan wares as compared to that of Amri wares should also be noted.

This pre-Harappan evidence in Sind and our awareness of the increasing cultural complexities in pre-Harappan Baluchistan, where many prototypes of Harappan traits occur, suggest that the Harappan civilization is but the latest phase in a long development (Childe 1952:183ff.).[5] It follows, then, that the term "explosive evolution," as used by Wheeler, is misleading. It would appear that the Harappan culture is the most Indianized, but I think that its essential roots are unquestionably Iranian. We can, in fact, envision two parallel, mutually influential developments occurring in the Indus Valley and in Baluchistan. The Indus Valley cultures became more and more Indianized, and this Indianization diffused to Baluchistan, both areas achieving greater cultural complexity as a result of these processes. Certainly the Indus Valley with its soil and water resources must have encouraged and supported denser populations. There, too, the native fauna and flora and the still hypothetical surviving forest cultures aided in the Indianization which is already apparent, for example, in pre-Harappan Kot Diji. The Harappan civilization as the last phase is the most complex of the Indianized cultures in the Indus Valley.

I have pointed out that the most elaborate cultural phase in Baluchistan, which in another context I have labeled "Zhob Cult," is among the latest in that region (Fairservis 1959:308, 330). In northern Baluchistan this phase is non-Harappan, but in its antecedents and some of its traits it is related to the Indus Valley. However, during Harappan times contacts appear to have accelerated, and the remarkable elaboration represented by the late Kulli culture and perhaps the late Periano Ghundai material of the Zhob Valley stem directly from these liaisons.

Archeologists and historians seem not to doubt that the Harappan culture is a civilization. Though the term has been bandied about to excess, a definition has now attained general acceptance (Braidwood 1952; Frankfort 1951; Kroeber 1957; writings of Childe). But it seems clear that we sometimes still tread on soft ground when we look for the origins and define the character of a civilization like that of the Indus Valley. About 1500 miles of Iranian plateau separate Mesopotamia from the Indus Valley. As Wheeler has pointed out, the "idea of civilization" crossed that region from west to east. Thus, as presently defined, we have "civilization" in the two widely separated areas.

But one might ask, why civilization did not occur between? Certainly there are sizable fertile areas in Baluchistan where men could and did concentrate in some magnitude. In Iran and Afghanistan there are several good sites suitable for the development of civilized communities, most of which are nearer to Mesopotamia.[6] On this basis it would seem clear that soil and water resources provide no greater impetus for the development of civilization than proximity.

Excavation in numerous sites of the Iranian plateau has revealed either the traits or their prototypes that make up civilization. We have evidence representing the work of specialists such as metal workers and potters, for monumental building (for example, the Amri dam), for wheeled vehicles (example, Namazga Tepe III), seal symbols, and potters' marks that suggest mutually intelligible symbolization (example, Quetta ware). Important, too, are the indications of extensive cultural contact; for example, pottery types such as Faiz Mohammad Gray ware and Quetta Wet ware are found all the way from Fort Sandeman in southern Kalat and Sind.[7] We are also increasingly aware of large irrigation projects and sizable settlements for which the term "town" might well be used.

In view of this evidence, it would appear that the seeds of civilization were planted all over the Iranian plateau as the result of the influence of the Mesopotamian area. I am not at all certain that civilization of a kind did not develop in Early Iran; however, there appears to be no doubt that a retarding factor operated to hinder its full flowering. The potters' marks generally remained potters' marks and the towns remained towns. The critical point is that civilization did develop in the Indus Valley not because that valley was in a unique position to receive Mesopotamian influences, nor because it uniquely possessed good soil and abundant water, for such resources occur also on the plateau. Actually, the Indus Valley is more remote than most of Iran from Mesopotamian influences. In other words, both the Iranian plateau and the Indus Valley stood an equal chance of developing civilization when the traits of civilization were diffusing. But something operated to select one area rather than the other.

It is obvious when we contemplate civilizations of the world that they are of different kinds. The emphases differ: in Egypt, upon local and state religion; in Crete, upon the mercantile; in Sumer, upon urban agriculture; and in Shang China, upon its warrior state. They are unquestionably unique, though they share much in common. The fact that their uniqueness can be so readily defined indicates that the basic traits of civilization centered around different factors. Thus, as Childe has pointed out, international trade and geographic position created Cretan civilization. Without that trade it is highly unlikely that mere stimulus or idea diffusion could have created a Cretan civilization in spite of the favorable position of the island in the Mediterranean. Similarly, the Sumerian development, created out of a need for intense centralization to control increasing social complexities, as well as water and soil resources in an arid area, died when that centralization failed to produce the unity necessary to survive in an international world.

The same causal factors that create a civilization also serve to identify it. Because anthropologists generally define a civilization as a complex culture, and use the word "intensify" to signify the heightening of cultural activity so as to produce this complexity, we might call the motivating factor the "intensifier." Accordingly, we might say that all civilizations have an intensifying factor that motivates them. In addition we could say that all cultures are influenced by a similar factor, but in the case of a civilization the intensifying factor attains a greater magnitude. It necessarily follows, then, that favorable geographic position is a prerequisite, but that the favorability of the position depends on the intensifying factor. Thus, in Iran, if trade had reached the Cretan magnitude, it is conceivable that civilization would have arisen there much earlier than it did because Iran was in a favorable geographic position.

The identification of the intensifying factor (Frankfort 1951:16)[8] that brought about the gathering of civilizing traits that produced the Indus civilization is thus a critical objective of our present researches. If we consider trade as the intensifying factor, we are then justified in comparing the Harappan civilization with the Cretan. We find that the Cretan civilization can be described as "dynamic, changing, and demonstrative of variety in its arts and crafts"—a summary that certainly does not fit our present knowledge of the Harappan. Further, serious doubt can be cast on any Harappan dependence on extensive foreign trade, because virtually all the raw materials used are found locally or at least in close proximity. The celebrated lapis mines of Badakshan, for instance, scarcely merit consideration in view of the poor quality and scattered quantity of lapis found at Mohenjo daro, as Marshall (1931) has observed.[9]

If we speak of an empire, as has been indicated, we are hard put to determine the kind of empire we really mean. Childe (1952:173-4) was bothered by this and could find no true parallel in the West. Obviously, we cannot mean a militant type on the order of the Achaemenid, Assyrian, or Aztec empires, because the simplicity of weapons, the lack of war machinery, and the absence of the usual oriental display of victories are very marked in the Indus Valley culture. Fortifications are attested, but some are earlier than the Harappan (Tharro Hill, Kot Diji), and some, such as those of Harappa and Mohenjo daro, are equally efficacious for defense against flood as against invasion, the former more likely than the latter in view of the site situation.

Political control does indeed require power in the hands of an authoritarian regime, and the use of citadels is almost inevitable. Here we can make comparisons with the urbanized agricultural civilization of Sumer, where political control was the natural result of the centralization of authority in order to make most efficient the control of irrigation and land. What qualities are involved here? First, we note that the city was a center in the cultivated area. The Sumerian farmer generally worked the fields by day and returned to the city environs by night. To a considerable degree the city control of the land depended upon proximity, i.e., the distance conveniently traveled by the city farmers to reach the outlying fields from the city center. Thus in a limited cultivable zone we have a multiplicity of cities and equivalent states. Interurban strife was commonplace, an inducement to centralization. Though religion, language, writing, and technology were generally similar, there was still a considerable variation in crafts, worship emphasis, and external contacts. Battles to gain dominance were accelerated by the struggle for resources and became international as the need for raw materials became acute. Sumerian expeditions to Iran, Arabia, and the Persian Gulf islands are well attested. We can assume, then, that the centralization of Sumerians, initially perhaps the result of agricultural need, was intensified because of military and industrial demand, until they lost control of their environment and thus collapsed.

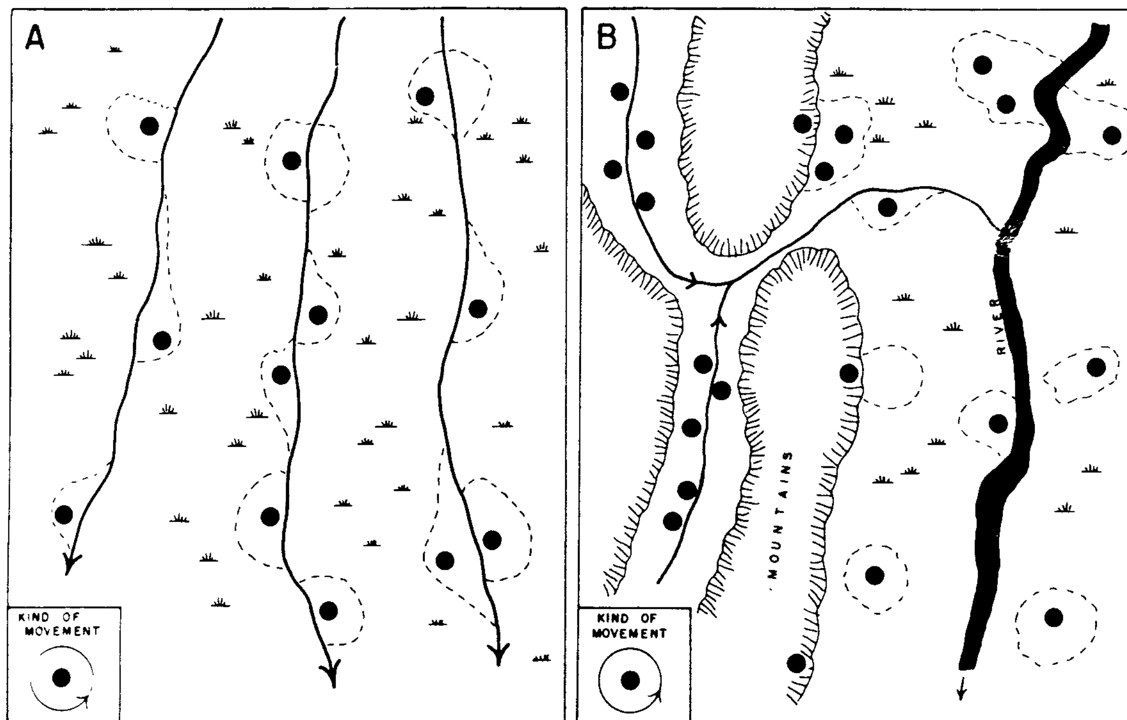


FIGURE 5.2. *A diagrammatic comparison of settlement patterns in Sumer and the Indus Valley. A. Settlement in pre-urbanized Sumer. B. Settlement in pre-urbanized Indus Valley. Dotted lines indicate the limit of cultivation; local movement centered around village and fields, i.e. settlement patterns nearly identical.*

Particularly in the later phases are we aware of dynamic progress which ranges from the improvement of writing and the formulation of laws to the improvement of metallurgy and the creation of exchange codes.

What a contrast meets our eye when we view the Harappan civilization in the light of Sumer (Figs. 5.2 and 5.3)! Most striking is the fact that in contrast to a multiplicity of urban sites we have a majority of village sites and only two, perhaps four, settlements large enough to be labeled cities.[10] If one draws a 30-mile circle with Mohenjo daro at its center, one finds such sites as Lohumjo daro, Kot Diji, and Jhukar encompassed. Further, in wandering north or south from the "citadel" of Mohenjo daro, one encounters any number of detached sites in each direction. If we consider the number of Harappan sites already discovered by Majumdar north of Lake Manchhar on his last expedition, I think we can have little doubt that many others occur along the western Nara branch of the Indus. Thus Mohenjo daro lies at the heart of a cluster of smaller settlements, and this cluster appears to become denser as one moves to the center. I suspect that this means that the Indus farmers were villagers through whose labors the urban centers were nourished. It may also indicate that there were probably no large canal systems or other monumental engineering works that required strong central control. Quite the contrary, it would appear that the flood waters of the Indus and the broad alluvium of the valley permitted widespread easy settlement requiring only limited irrigation and did not provide the impetus to centralization any more than did warfare (Adams 1960a).[11]

In fact, as agronomists have discovered during the past several decades, the problem of Sind is not lack of water but too much of it. Salinity and flood are commonplace and can occur anywhere between the eastern and western Nara branches of the Indus. Thus the problem is not to bring water to the fields but to take it away, an engineering task that almost exceeds the skills of the twentieth century, and one certainly not solved by the Harappans. Irrigation apparently was not a true centralizing factor as it was in Mesopotamia. Accordingly, we can assume that the life of the Indus farmers centered on the village and not

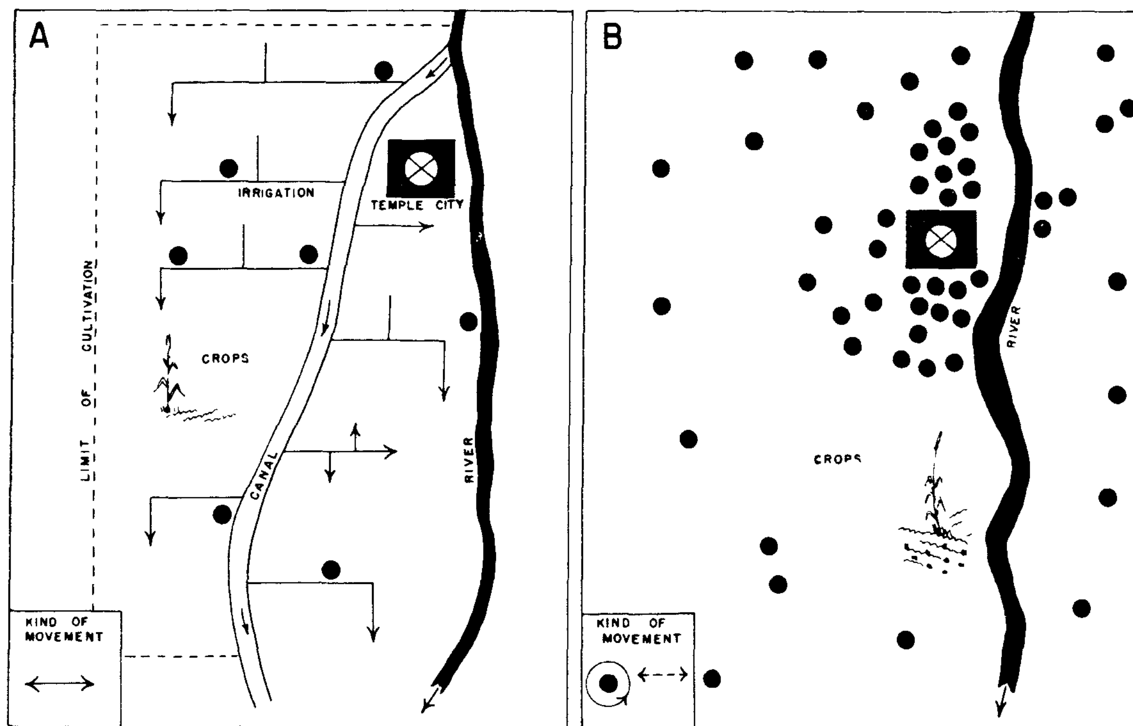


FIGURE 5.3. *Diagrammatic comparison of settlement patterns in Sumer and the Indus Valley. A. Urbanized Sumer. Population movement between fields and city. B. Urbanized Indus Valley. Principal population movement centered about village and fields; occasional movement to urban center*

the town or city and, under such conditions, the dispersal of fields was at a maximum (Figs. 5.2 and 5.3).

This assumption is critical for it places the majority of the population at a distance from the known urban centers. It renders the spread of new ideas more difficult and the rate of progress considerably slower, primarily because human interaction is given limited scope. Further, if the Indus River region were not on the highroad between east and west, as its location on the far side of the Iranian plateau indicates, alien ideas would only filter in and be further filtered by a decentralized situation. I believe these factors account for a considerable part of the seemingly static quality of the Harappan civilization.

At this point in our understanding, we find that villages of the Indus Valley differ in no way in essential features from those of the Iranian plateau. However, the intensifying factor that created civilization forced some portion of village settlement to urbanization. Here again we search for comparative material. Our search requires a comparable civilization possessing a generally static aspect in the arts, crafts, writing, and technology, with a basic village economy. Unquestionably, we have a good parallel in Egypt, especially in the Old Kingdom, but, as Piggott (1950:140-141) has pointed out, there are equally good parallels in pre-Columbian Mesoamerica. In Egypt religion appears to have been the intensifying factor. Art, writing, architecture, and even social form (at least in Egypt) were religion-oriented. As Childe (1958:95ff.) acknowledged, the control of arts and crafts was of such strength that they remained static through centuries. Whereas in Egypt a certain degree of state control contributed to this static quality, the religious forces were dominant. The resistance of ancient Egyptians to change, in part the result of isolation, has been noted by many (for example, Wilson 1946:31ff.). Similarly, the general uniformity and considerable longevity of Maya style might be accredited to a combination of isolation and religion orientation. Hence, the basic settlement pattern in the Maya area appears to have been one of scattered dwelling units radiating in diminishing number from a ceremonial center (Bullard 1960:355ff.).

In view of this evidence, it appears that religion was the intensifying factor that created and gave form to the Harappan civilization. I submit that Mohenjo daro was almost purely a ceremonial center and that its functional intent was similar to the centers of the Old Kingdom Egyptians and the Mayans. Consequently, I think there are grave doubts that trade had much influence on its location or its upkeep. I suspect that the political or military orientation involved in the term "empire" is unwarranted by the present evidence.

The character of Harappan religion is fundamental to this thesis. We are fortunate in possessing a body of useful facts in this regard: (1) the occurrence of female and bull figurines in all Harappan sites and in a large percentage of sites in Baluchistan; (2) the construction of large buildings at prominent points and their association with drains, baths, cisterns, storage areas, and the like; and (3) the formality in appearance of the seals as a whole and the character of the seal writing, apparently ritualistic in content.

These three elements are not individually conclusive. Each might be otherwise interpreted, for example as: (1) toys; (2) secular structures; (3) ownership stamps, but as a whole their religious character seems manifest. Some hint of the ritual nature of the Indo-Iranian borderland religion was revealed by our excavations in the Quetta Valley in 1950-1951. Leslie Alcock unearthed a portion of a large platform situated on the top of the mound of Damb Sadaat (Mian Ghundai). Stone drains were located in the midst of this mudbrick structure, and there is good evidence for the existence of buildings on top (Alcock in Fairservis 1956a:214-216). There is also some evidence for a massive outer wall which may have enclosed the central structure. Associated with this complex are the well-known Zhob mother-goddess and bull figurines, some of the latter painted with ovoid markings between the horns (Fairservis 1956a: Fig. 18). Bovines with painted bodies and horns also occur. A small stone cache in one corner of the platform at the foundation level contained a detached human skull.

Apparently this Quetta material, representing the last major prehistoric phase in the Valley, is of a religious character. On the basis of this evidence we can readily envision ritual bathing, human and animal sacrifice, and the intentional placing of ceremonial structures at the highest point in the village.

There is evidence for the widespread existence of this religious complex, which I have labeled the "Zhob cult," in Loralai, the Fort Sandeman area, and in southern Afghanistan (Fairservis 1959: 308, 330; 1958:510; and 1952:14). Contemporary sites in southern Baluchistan reveal that the principal Kulli culture occupations were the most recent. Bull figurines and goddess figurines are found at the type site and at Mehi on the upper slopes of the sites. At Kulli,

at the top of the site, there is evidence for a large building. Kulli figures are found in rooms of this building.

In the course of my recent field trip to Las Bela district, the discovery of an enormous complex of ruins along the middle Porali Valley north of Bela town gives us further evidence (Fig. 5.4). Here the fertile plain, an area of well-cultivated farm land some 10 miles square,

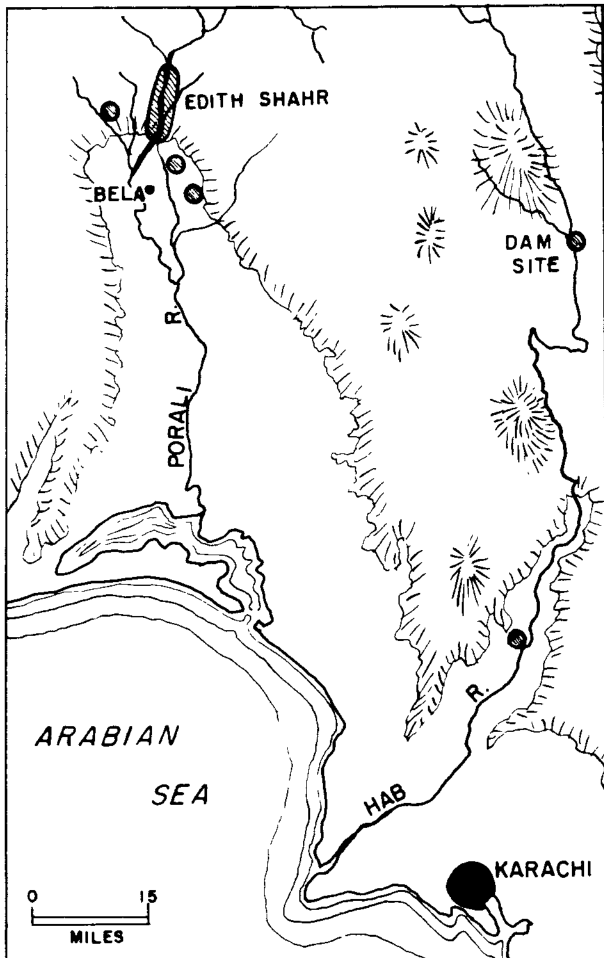


FIGURE 5.4. *Map of sites investigated by the American Museum of Natural History expedition, 1959.*

is located in the midst of the braided river area surrounding Bela town. This area diminishes until, about 10 miles north of Bela, it virtually fades out as the valley narrows. For about 8 miles north of this point the Porali flows south, out of the canyon country that marks the border of Kalat, between low gravel banks formed by old terraces and by talus from the surrounding hills. These terraces are arid, inhospitable to agriculture. However, on these gravel terraces on both sides of the river, from the border of Kalat to the beginning of the cultivated area, there occur a great many contiguous sites representing two complexes, both of which are generally found together. The earlier, Complex A, is clearly of late Kulli affiliation, and both the characteristic pottery and the figurines are found in some abundance (Fig. 5.5). Notably the Complex A people took advantage of the readily available stone boulders found in the talus of the area and constructed formidable foundations for their buildings. Some of these walls now stand approximately 6 feet in height; the whole complex is in such a state of preservation that the building plan, settlement layout, streets, ramps, and doorways are clearly observable. Initial observation revealed a consistent plan: Large structures, on the bluffs overlooking the river, consisted of ascending stages receding as they rose in ziggurat fashion, and crowned at the top with platforms supporting brick buildings. These platforms were reached by ramps or steps. There are two good examples of drains let into the body or edge of a platform. Surrounding these high structures, some of which rose over 30 feet above the surrounding area, are complexes of structures with intervening lanes or streets, stone-paved floors and drains or cisterns located in these floors, apparently in small chambers. Beyond these structures groups of rectangular buildings, some over 70 feet long and compartmented, present a formal appearance, suggesting hierarchial living quarters or perhaps tombs.

Aside from the structural and artifactual parallels to those of the northern cultural phases, this Las Bela Complex A culture presents one very important feature. The location of the complex north of the cultivated area and the concentration of specialized population, indicated by the 8-mile stretch of ruins, are a clear demonstration, it seems to me, of a ceremonial hierarchy supported by farmers. Actually, as Stein (1943b) already had discovered, there are several contemporary village sites in the cultivated area. Thus the so-called "Zhob cult," which includes this Kulli material, can be summarized as being involved with probable fertility symbols in women and cattle, the building of large structures in prominent places, ritual bathing, and support by a farming populace.

From the typological view it would appear that the late Kulli culture, Damb Sadaat III, the pertinent levels of Loralai-Zhob, and Afghan sites are all about contemporaneous and range from slightly earlier than,

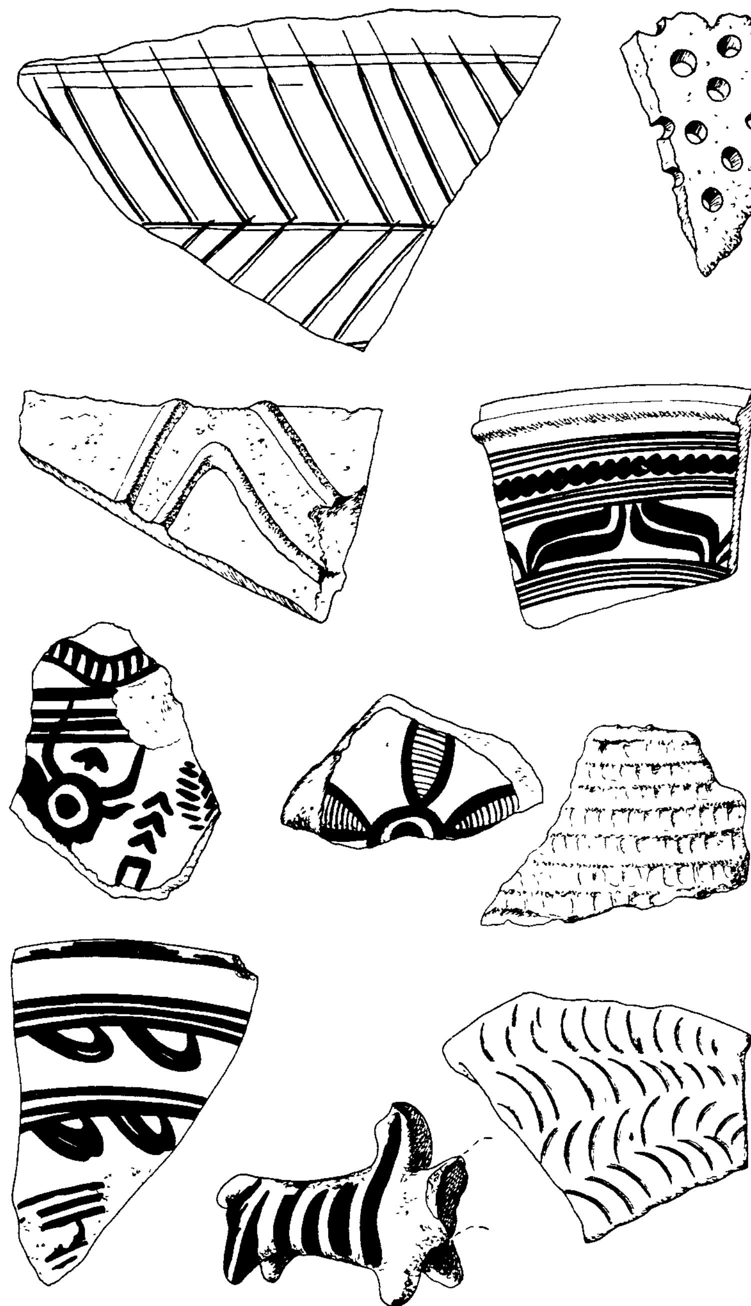


FIGURE 5.5. *Pottery and figurines of Complex A.*

to full contemporaneity with, the Harappan of Sind (Fairservis 1956b:153–156). There is no question at Mohenjo daro that there are strong similarities to the Zhob cult. The elevation of formal buildings, drains in high places, and the presence of figurines in association are undoubtedly close parallels.

It is, however, the seals that furnish strong evidence for a formal religious cult and give us a further clue to its nature. Already well known are the "Sivaite" elements, including the multifaced god, the yoga position, the horned deities, the tree figure, and the "Lord of Beasts." Familiar, too, is the multiheaded creature with the bovine body.[12] But by far the commonest subject represented on the seals is a bull,

with horns thrust forward and curving upward. This representation goes beyond merely suggesting the powerful bull of the Mediterranean area; it may as well depict the extinct aurochs. It is noteworthy that in the majority of examples in which this animal occurs, a flowing, heart-shaped design, upside down, is engraved upon the shoulder. Other incised areas include the snout, horns, and neck; these vary from example to example. I suspect that these represent the painting of sacred marks which play the same role as those upon the bull figurines of the Zhob cult. One also notes the repeated occurrence of a double stand, manger, or symbol in front of this animal which appears very rarely elsewhere.

In view of the common occurrence in historical and modern India of sacred cattle, branded or painted with sacred emblems, and on the basis of the foregoing, I do not think it unwarranted to assume that these are indeed sacred cattle dedicated to some religious entity symbolized by the objects before which the beast stands.

While much of this may be obvious, and certainly few have denied the religious aura of the Harappan civilization, it seems very clear that our present evidence so overwhelmingly supports the ceremonial side that it is difficult to find an equal place for other possibilities. In the bull represented on the seals, we are dealing with an animal that not only must have been a principal factor of the religious scheme but a significant entity in the economy—perhaps the *raison d'ètre* for its sacrosanct character. It is worth quoting Robert Wallace who, as Professor of Agriculture and Husbandry at the University of Edinburgh, visited India in 1887 to study methods of improving the breed of Indian cattle. In the course of his work he arrived at a conclusion important to our present investigations:

"The first, and by far the greatest and most important object for which cattle are reared, is to provide the motive power required by the ryot to cultivate the soil, to raise water for irrigation and minor needs, when wells form the water supply, also to convey by cart, or more rarely by back-load or burden, his produce to market" (Wallace 1888:1).

Anthropologists and historians have long known the relation between cultural growth and the control of energy. Leslie White (1959) expresses it as: "Culture advances as the amount of energy harnessed per capita per year increases, or as the efficiency or economy of the means of controlling energy is increased, or both."

Is it possible that the origin of the sacredness of cattle in India derives from the advantage cattle gave man in his battle for control of his environment? The prevalence of the dominance of goats and sheep over cattle in the early Iranian village cultures probably indicates the minimal use of cattle as a food animal. Cattle as draught animals made possible the cultivation of Iranian fields, limited though most of them were. But one wonders what kind of symbiosis between cultivation and cattle grazing was achieved. The more cultivation the less natural grass and the greater the need for fodder; accordingly either a human or bovine population increase affected the food margin of the other. One could not be abandoned for the other. Unlike sheep and goats, the grazing of which can and most frequently does take place outside the bounds of cultivation, cattle are generally dependent on exactly the same soil resources as man. In effect, man needs to increase his cattle to open more fields, but he cannot have those additional cattle without additional fields – a rural paradox of widespread significance.

It is clear that if cattle and man in symbiosis are major factors in the development of advanced village cultures, the environment sets bounds to that development If increase in the amount of energy harnessed each year leads to the development of civilization (without extenuating factors), the bulk of the villages of Iran could not achieve that cultural level.

However, prehistoric man in the Indus Valley had almost limitless boundaries. The same grassy plains that supported the rhinoceros, elephant, and buffalo were ideal for the breed of western Asiatic cattle that presumably first descended there. The spread of Iranian prehistoric cultures in the Indus Valley resulted from the situation in an almost limitless favorable ecology, at least compared with that of the plateau. Here cattle must have increased in great numbers, probably in part adapted by interbreeding with humped bovines indigenous to the region. The critical point is that man harnessed this waxing energy font and so increased his food resources. As population increased, and such increase must have been rapid, the demand for new pastures and fields was a motive for movement north and south. By Harappan times this movement was in full swing, primarily because the limits of the original Indus plains had been reached, and food production was, if anything, falling short of the needed quota.

The dependence on cattle for energy is thus tightly linked with the idea of increase, and thus to fertility. It does not seem difficult to envision an original sacred

allusion to cattle among early agriculturists. The bull figurines of the plateau sites appear to suggest this relation. But the Indus Valley cattle may well have become a major feature of the religion as a direct outcome of their essential role in food production. The Zhob cult of Baluchistan is probably a direct reflection of the religious orientation of the Indus Valley. Cattle seem to have played an increasingly important role in Baluchistan. In Sur Jangal III, for instance, cattle almost completely replaced goats and sheep.

The picture of a religion-oriented civilization is not a really clear one. We can assume a numerous priesthood and a correspondingly large food contribution by villagers. We might assume that an essential rite of priesthood was purification and thus account for the baths and drains. Much has been written, and by many, on these matters and needs no repetition here. However, one point should be emphasized: that is, the effect of religion on secular culture traits. If we compare religion-oriented civilizations such as the classic Maya, the Egyptian, and the Harappan, the phenomenon of generally static trait style, in time and space, stands out as an important characteristic. This static quality appears to reflect a stable productive economy and a paucity of interaction with other cultures outside the culture. Change, therefore, is given little basis, as the status quo has favorable advantages.

This cultural characteristic gives us some understanding of both the time span of the Harappan civilization and the reason for its fall. Anyone who has visited Mohenjo daro will realize the precarious situation of the site, close to the banks of the Indus and continually in danger of flooding. It is doubtful that the depth of deposit here represents a great span of time. Flood-caused destruction and rebuilding could have taken place in 25 years as well as in 250. In this regard, an examination of the Harappan levels at numerous village sites in Sind reveals a surprisingly narrow band of occupation as compared to earlier occupations. One thinks of sites such as Kot Diji, Dabar Kot, and Amri, as well as the group around lake Manchhar. In any case, I suspect that the focus of the Harappan culture was in northern Sind, Kachhi, the Bugti Hills, and perhaps the Derajat, especially in view of the recent reports of large Harappan sites in these areas.[13] A degree of contemporaneity with the last phase of the Amri is, therefore, not out of the question. However, I am not convinced that the Harappan of Sind at least represents an occupation by that culture of anything close to the thousand-year span which is accepted at present. On the basis of the foregoing, I would expect that it was nearer 500 years. The static quality previously noted may represent a shorter cultural life, and the slight phases illustrating gradual shift in style and technique, which closer study is now revealing, emphasize that Harappan culture was undergoing an evolution. Earlier I pointed to the coincidence of distribution between the wheat-growing regions of India and the presently known Harappan sites. It has been generally accepted that the Harappan occupations at Harappa, Rupar, Alamgirpur, the Bahawalpur area, Lothal, and the Narbada represent late phases of the culture, though the details of these phases have yet to be clearly defined. At Mohenjo daro and at Chanhu daro there is clear evidence for progressive deterioration in the latest phases, as Sir Mortimer Wheeler has described. If we assume that Mohenjo daro is within the general area of the earliest Harappan cultures, it is obvious that that culture moved generally north (northeast, more accurately) and south from its place of origin and that, as deterioration occurred at the source, movement to the limits of the culture area took place. It is of great interest, though it is as yet not fully determined, that village sites in Sind, such as Kot Diji and Ghazi Shah, demonstrate the earlier but not the later phases of the Harappan. If confirmed, it would indicate that the latest phases of the Harappan in Sind existed primarily at the ceremonial centers and would accord with the assumption of the existence of a sacred center which gradually became obsolescent as populations moved away and/or support for the faith waned. The geographic extent of the Harappan civilization becomes less formidable then, if we consider that occupation of vast areas was not contemporary.

Wheeler has pointed out that the probable cause for the "collapse" of the civilization is the "wearing out of the landscape." I would interpret such a collapse as occurring because the growing population of men and cattle spread to the limits of the feasible cultivated area so that the symbiotic balance was upset when no further expansion was possible, at least within the original areas of settlement. Unquestionably over-cultivation, over-grazing, salinity, and floods took their toll, but I doubt that tree-cutting for fuel had as significant a role in the change of conditions as Wheeler indicates. I can find no evidence that the kind of trees involved affects the rainfall by means of transpiration, particularly in a land where rainfall sources are outside the provincial limits. The removal

of the grass cover for cultivation, the decrease in fertility of the soil due to over-use, and the fluctuations of salinity because of the shift of the riverine situation are far more serious matters, in that they affect the food production more than does the naturally sparse rainfall dictated by geography.

By the time these spreading movements reached their zenith, we can assume that the Harappan farmers almost invariably followed a specific pattern. This pattern seems to have included the placement of villages near a good source of moisture, the establishment of a ceremonial center once village settlement was large enough (usually involving the construction of a bund for protection against flooding), the laying out of dwellings on a rough grid plan, and the building of a structure or group of structures for ceremonial purposes. Of course, we may discover that such centers became rarer towards the extremes of the Harappan expansion, probably as the result of greater dispersion of settlements. In such cases it is likely that a ceremonial area was designated within each village—a speculation not unwarranted in view of the apparent maintenance at all sites of the more formal aspects of the Harappan, including the seals, figurines, and vessel forms, and possibly the evidence for such shrines in modern village India.

A point to consider in this regard is the possible motivation involved in elaborating religion, that is, the increased need for religion as population increased and the perils of famine thereby intensified. In more prosperous situations, as, for instance, at the time of the initial Harappan settlement of East Punjab and Gujarat, when famine and similar disasters were not an immediate threat, the religious needs were perhaps not so acute and a reason for large religious centers was minimized, i.e., the urge to centralization waned.

In other words, as the balance between men and nature becomes precarious, religious activity intensifies. If this axiom is valid, then we have a rough gauge for estimating a stage in early civilization, i.e., the more temples, rituals, and similar paraphernalia, the more uncertain the economic situation. On this basis, the Harappan stage would represent the most economically precarious in the evolution of the food-producing cultures of the Indus Valley, a seeming contradiction to the stable economics indicated by its style character.

The word "collapse" is perhaps too dynamic a term for the end of the Harappan civilization. Perhaps "eclipse" is a better one. Wheeler, Subbarao, and others have pointed to the occurrence of Harappan elements in "post-Harappan cultures," including Jhukar; a considerable survival is indicated in Kathiawad. Subbarao, in a brilliant regional analysis, has pointed out the survival patterns in North Central India, his so-called proto-historic tri-junction (Subbarao 1958:101), suggesting "the possibility of a Harappan survival in the peripheral regions at the east, west, and south of the main Indus basin." All this indicates a shift of the cultural focus away from Sind and a long survival in the southern and peripheral areas. It does not explain certain occurrences. One is the failure of the Harappan farmers to move into the middle and lower Ganges and adjacent areas when they were readily accessible. In fact, nowhere is the Harappan known beyond the bounds of the wheat-millet-growing regions. The answer is that apparently the more tropical areas were unsuited to the cultivation of wheat-millet. The Harappan farmers and their local descendants had reached the limits of their range.

It is with no little interest, then, that we find the next substantially traceable occupation of the Ganges region represented in the so-called Gray ware period, by a people using rice as well as the wheat cereals. The movement of Gray ware cultures eastward along the Ganges is indicated by finds such as those at Kausambi. Copper-hoard sites have been found as far east as the Mahanadi River in Orissa. These two cultural entities provide evidence for the penetration of the lower Ganges region from the west; both are post-Harappan in time (Subbarao 1958:151ff.).

The continental situation in the earlier half of the second millennium B.C. indicates that both in China and India developed agricultural groups were pressing up to the boundaries of tropical Asia, which were already inhabited by hunting cultures and probably by primitive agriculturists. These southern areas were extensively occupied more and more after 1500 B.C. By shortly after 1000 B.C., full-scale clearing and cultivation were penetrating deep into tropical Asia as far, at least, as the Indonesian islands. Rice was the basis of this elaborate southerly agriculture. Although the details of this dramatic shift are lacking, it is clear that grain agriculture until after 1500 B.C. had been confined to a narrow band along the flanks of mountains and deserts and in the river valleys south of about latitude 40° N. The civilizations of Shang and Harappa were foci of cultural developments within this band. After 1000 B.C. these foci shift into or depend on another geographical region, as expressed

in the waxing influences of kingdoms in South China and the rise of the Ganges civilizations. The coincidence of the development of the cultivation of rice and the rise of these new foci is thus significant and wide-ranging. Again the details presently escape us. However, I suspect that the final blow to Harappan civilization as such was the opening of the rice areas.

The difficulty is that we are dealing with cultural events that as yet we can only poorly grasp. For instance, it becomes increasingly clear that in Baluchistan during the Damb Sadaat III-Rana Ghundai-Kulli period there were sizable populations in every watered valley. The contemporary Harappan of Sind was even more populous. But if the latter relieved their economic stress by moving to fairer pastures, what happened in Baluchistan? Did the Baluch farmers occupy the already impoverished Sind region, as suggested by the Jhukar pottery, or did they simply deteriorate in place—a fact for which we have no good evidence as yet? On a wider scale, the evidence in the Iranian plateau region, especially on the south, indicates that populations decreased markedly after about 2000 B.C. and were not immediately replaced. Was this the result of the eventual deterioration of each landscape or the pressure of invaders? I suspect the former is the answer. Certainly enough remained to preserve an immemorial way of life from Kerman to Sind and beyond. Nevertheless a kind of cultural vacuum must have developed across which accelerating international contacts gave news of greener pastures. The Aryans, Megalithic folk, the Sialk B carriers, and so on, moved across impoverished land where cultural retardation did not occur as it had in pre-Harappan times. For the Aryans, the Ganges, with its populations and established cultures, was a new and rich homeland; for others, the Deccan. For the Harappans it was a gradual submergence into a new cultural form, but fundamentally still essentially the village-urban settlement patterns of old.

To summarize, I believe that there is not sufficient evidence to suggest that the climate at the time of the development of the Harappan civilization was essentially the same as that of today.[14] Accordingly, we can understand the physiographical changes that must have occurred when full-scale agriculture developed in the region in pre-Harappan times as the result of the spread of farming cultures across the Iranian plateau. On this plateau the populations lived in villages, but these villages tended to cluster in certain areas to take advantage possibly of united efforts to maintain dams and irrigation under arid conditions. In the Indus Valley, however, such clustering was no longer necessary for these reasons, and the result was a dispersement of villages in Sind during pre-Harappan times. With the use of cattle as a means of increasing the sources of energy necessary for the wide cultivation of the grassland, a symbiosis developed on a large scale which brought large areas into the food-production scheme. An Indianization process developed as a result of contact with older forest hunting cultures, primitive agriculturists, and the Indian environment, thus insuring a special civilization style. As the populations of cattle and men increased, the religious needs provided reasons for centralizations that served to intensify certain interactions so as to create civilization from seeds already planted by stimulus diffusion. Ceremonial centers became a feature of the last major prehistoric occupation of Sind (Harappan) and most of eastern Baluchistan. The village economy provided the support for the centers. Cattle, particularly the bull, were prime factors in the religious cult, with fertility as the assumed motivating force. The religious character of the Harappan civilization underlies its formal and static aspect. However, in Sind general Harappan occupation was limited in time, as the cultivated boundaries of the valley were at their most far-reaching extent during that period. The urban aspect of the civilization was in contrast to that of Sumer-Akkad with which it had only chance and occasional contact. In fact, in the Indus Valley isolation was probably an important force in maintaining the status quo.

The economic needs of an expanding population gradually brought about the movement of the Harappan culture to the northern and southern areas of the Doab and to Gujarat-Kathiawad. Gradual economic and cultural deterioration in Sind and increasing settlement on the extremes of the grain-growing regions characterize the late phases of the Harappan. The exhaustion of soil resources in the Indo-Iranian borderlands caused a drastic falling off of population and a kind of cultural vacuum. In addition, the development of rice cultivation after 1500 B.C. changed many of the premises on which the Harappan character of these early agriculturists was founded, and the culture as such disappeared, except in localized situations. The influx of new peoples across the Iranian plateau after 1500 B.C. was virtually unopposed. These new groups provided numerous ideas and energies that contributed to the development of later civilization in

India, but they were not primarily responsible for the collapse of the Harappan civilization. They merely eclipsed the remnant which stood in their path and built on an already Indian foundation.

A point that helped to initiate this essay derives from a personal view that the Harappan civilization is basic to the development of the specific character of India. In the broadest sense it becomes difficult to reconcile Nehru's concept of India as a land of villages and religion with the present ideas of a unified Harappan empire, West Asiatic in aspect. On another plane, we are constantly faced with the problem of defining materially what we mean by civilization. The problem becomes particularly difficult when we find essential differences in form when comparing contemporaneous ancient civilizations. It is clear that the Harappan civilization is not a watered-down Sumeria with Indian embellishments, nor a Sumerian civilization far from home. It is definitely Indian in its important features. One of these features, it appears, is the peculiar situation of a society possessing civilization but dwelling largely in villages with ceremonial centralization, and decentralization for almost all other purposes. What effect does this situation have upon the evolution of the culture involved? What happens to new ideas and techniques when they are introduced into this cultural ground? We can arrive at conclusions by inference, but we need concrete evidence. Indian culture, especially that of Hindu India, it seems to me, furnishes enormously important evidence that, despite the pitfalls of hindsight, we cannot ignore. In Hindu India we have villages and ceremonial centers in quantity. Local trade is on the village level, but town bazaars are typical. Was Mohenjo daro a center for local trade as well as religion? While we freely search for contacts with Sumer, do we understand the interactions that occurred at the village level? What was exchanged between villages? I am more than ever convinced that to know more about the Harappan culture we must know more about Indian village life. Perhaps then we can hold a glass and see more clearly the dynamics of the Harappan past, dynamics which most surely were motivated by Indian values.

Much of the foregoing thesis is admittedly speculative, but it may be of some importance as a stimulant to further research and increasingly valid conclusions. It seems time for those who are involved to re-assess what we know, and what we think, and hope that ensuing discussion will bring us nearer the truth.

My gratitude should be expressed to Dr. F.A. Khan, Director of Archaeology, Government of Pakistan, for the opportunity to pursue inquires in the field, and to the late and deeply lamented Lauriston Ward of the Peabody Museum, Harvard University, for scholarly foresight in the problems of the Indo-Iranian borderlands and for his constant encouragement, without which these inquiries would not have been possible.

NOTES

[1]Note the North African affinities in the Imperial Gazetteer of India (1909:179)

[2]Primarily wheat and millet as opposed to rice

[3]The Las Bela material will appear in a forthcoming publication, which will report on the findings of the field season of 1959-60 in Las Bela District. Editor's note: see Fairservis (1971:185-204) for this report

[4]The problem of the correlation of Kot Dijian levels is not yet solved, but F A. Khan has already hinted at similarities to pre-defense material at Harappa Editor's note: see Mughal (1973) in this volume for a current discussion of this material.

[5]The pre-defense level at Harappa is also probably indicative of this development.

[6]For example, the Caspian littoral, the Middle Hari Rud, the Oxus Basin, and the Kandahar area.

[7]Note also the widespread distribution of similar figurine form, brick size, and pottery designs.

[8]Frankfort calls the character of a civilization its "form" described as "a certain consistency in orientation, a certain cultural style." This "form" might be regarded as a passive definition of the intensifying factor which I regard as an active ingredient rather than the "shape" apparently implicit in Frankfort's definition.

[9]The seal evidence from Mesopotamia and possibly Bahrein is not demonstrative of extensive trade but only of occasional contact, as far as I can see—not an unexpected phenomenon in view of the accessibility of each area.

[10]Mohenjo Daro, Harappa and perhaps Judeirji daro.

[11]Adams deals in some detail with the problem of urbanization. Storage is regarded as a significant factor by Adams. It may well have been so in the Indus Valley as there is evidence for warehouses and large jars, but storage can be localized in villages too. The importance of excavation in Harappan village sites is further emphasized.

[12]Most of the seal subjects are depicted in Marshall (1931).

[13]This focus would be the epi-center which Subbarao (1958) has pointed out as a necessary identification for understanding the spread of the culture.

[14]Evidence for the similarity of modern and ancient climate accumulates. For example, refer to Chowdhury and Ghosh (1951). Editor's note: see other papers in this volume for perspectives on the ancient climate of the greater Indus Valley

The Origin, Character and Decline of an Early Civilization

WALTER A. FAIRSERVIS, Jr.

INTRODUCTION

The Harappan civilization which arose in the Indus River Valley toward the end of the third millennium before Christ (2300 B.C. to 1700 B.C.) can be regarded as the first of the manifestations of civilization to occur outside western Asia and Egypt. Since geographical remoteness tends to eliminate direct contact as the probable reason for its existence, its origins, character, and demise are therefore of no little scientific interest. Its very remoteness should allow us, in almost clinical fashion, to analyze this civilization as a cultural manifestation the complexities of which are obscured only by the necessarily incomplete archeological record. The primary nature of the phenomena of civilization that characterizes the past 5000 years of human existence can be reasonably probed if we utilize the opportunity presented to us in the extensive remains of the Harappan civilization. A beginning of such an examination has been made and is the purpose of continued researches by a number of scholars. On the basis of present research we may ask and expect qualified answers to questions as to the origins, character, and decline of the Harappan civilization. The evidence at hand indicates that we are obtaining a much clearer picture than has hitherto been available, a picture that reveals a great deal as to the mechanisms by which a civilization may have developed and by which its character and history are specifically determined.

The present paper is an attempt to summarize some of the possibilities inherent in the newer archeological record. It is based on recent efforts made during three field seasons, 1959-1960, 1961-1962, and 1964. Its principal aim is to provide broader views of the problem than those normally found in the literature. The evidence seems capable of a number of interpretations; it is timely perhaps to view it from the point of view of new perspectives.

This paper owes much to discussions with Mr. (now Dr.) Gregory Possehl. Part of the funds for the field work on which this paper is based were received from the Bollingen Foundation, the Wenner-Gren Foundation, the National Science Foundation (Grant GS-533), and the Public Law 480 Program of the United States Department of State and the Graduate Research Fund, University of Washington. My wife drew the illustrations.

THE SETTING

The Indo-Iranian Borderlands consist essentially of that portion of the eastern Iranian plateau that lies south of the Koh-i-Baba ranges of central Afghanistan as far as the Arabian seacoast, including the southern Punjab and Sind portions of the Indus River Valley. Baluchistan lies at the heart of the Borderlands and serves as the link between the Seistan Basin at the boundaries of Iran and Afghanistan, southern Iran, and the Indus River Valley which is in West Pakistan and is subcontinental in character. The Borderlands consist largely of desert-mountain terrain, nine-tenths of which is barren most of the year. Annual rainfall ranges from almost nothing in Seistan to less than 8 inches in eastern Baluchistan. In contrast to the barren terrain of the Makran, Kharan, and Chagai or western portions of Baluchistan, much of eastern Baluchistan, especially the old states of Las Bela and Kalat and the districts of Zhob, Loralai, and Quetta-Pishin, are characterized by sources of riverine water, springs, and

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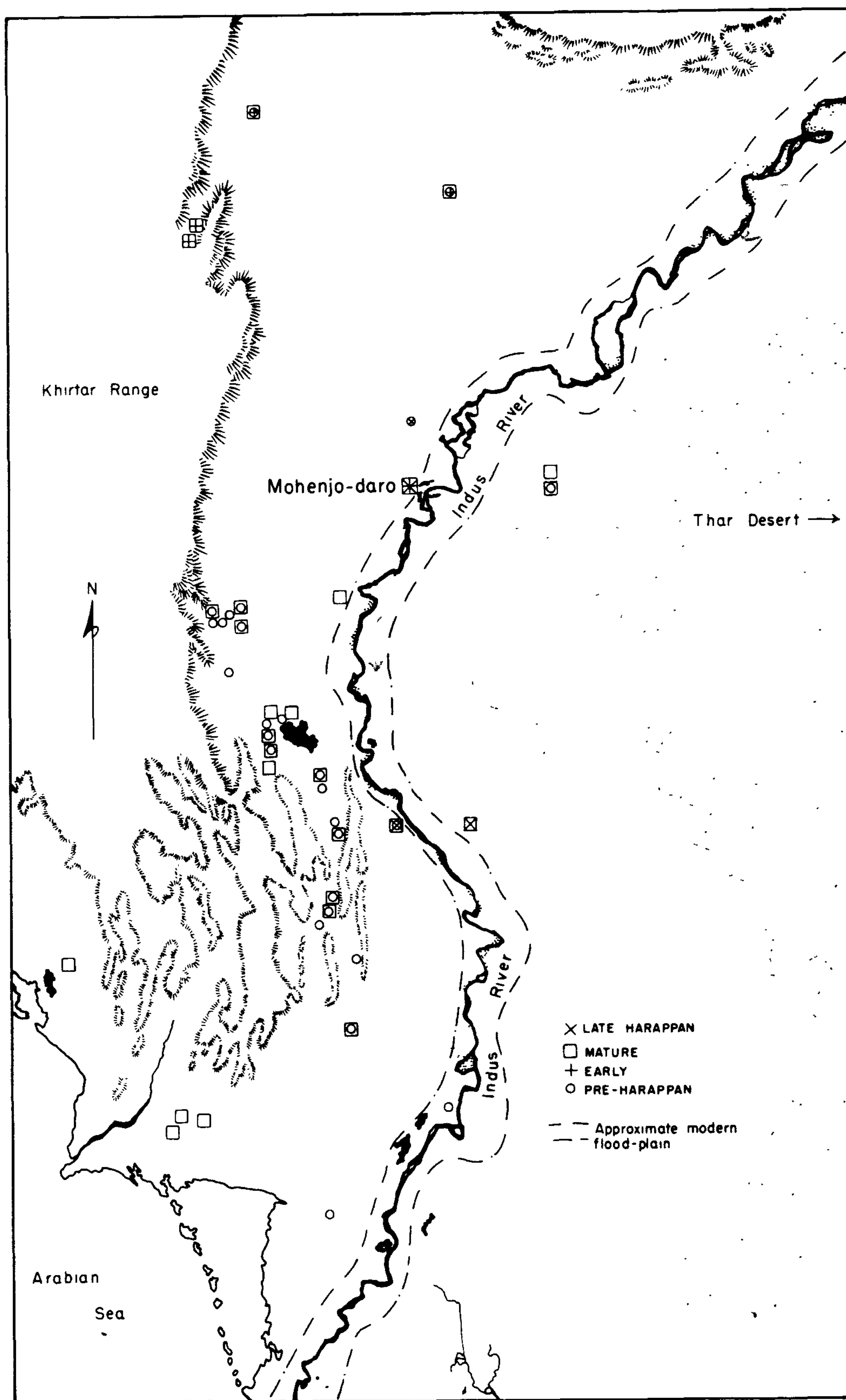


FIGURE 6 1. *Outline map of Sind in the lower Indus River Basin, showing the location of pre-Harappan and Harappan sites and their relative chronological position.*

perennial wells. As a result the population of Baluchistan is mainly situated in the east, on and about patches and ribbons of productive alluvial land.

The Indus River Valley is an enormously fertile alluvial plain. In spite of riverine vagaries and human misuse, it is one of the best agricultural regions of the world and is the backbone of the modern state of West Pakistan. Its flat, generally featureless plain contrasts with both the mountainous country of Baluchistan on the west and the sandy hills of the Thar, or Great Indian Desert, which bounds it on the east (Fig. 6.1).

The Indo-Iranian Borderlands are therefore not unsuited to human settlement; in fact they are much better endowed for this purpose than most of the Iranian plateau. The moderately fertile uplands of eastern Baluchistan and southern Afghanistan form the western boundaries of the Indus River alluvial plain. The evidence indicates that the interaction of the inhabitants in the uplands and those in the plain was essential to the development of civilization in the subcontinent.

ORIGINS

More than a decade ago Sir Mortimer Wheeler, dean of subcontinental archeology, pointed out that researches among the Indo-Iranian Borderlands were like philately. Investigators caught up in a maze of decorated potsherds representing the village cultures of late prehistoric time were guilty of creating schemes that summarized the story of that critical period and region as if it were simply an arrangement of sets of stamps in a collector's album.

However, since Wheeler made this statement, we have seen much progress in archeological research in the Borderlands. Where, at one time, every survey and excavation more often than not produced new materials unexpected by researchers, we have now reached that stage at which the material uncovered by archeologists is repeating itself. The stratigraphic sequence and the typological relationships of artifact assemblages are falling into a pattern at once locally meaningful and, in the larger perspective, of significant importance to the study of ancient civilization. Among the more important recent researches, those of the following should be noted: Beatrice de Cardi, central Baluchistan; Jean-Marie Casal, excavations at Mundigak (southern Afghanistan), Amri (south-west Sind), and Nindowari (southern Baluchistan); George F. Dales in Makran; Walter A. Fairservis, Jr., and associates in Quetta, Zhob-Loralai, southern Afghanistan, Seistan, southwest Sind, and Las Bela; F.A. Khan at Kot Diji, Bhawalpur; R.L. Raikes in surveys of Kachhi, Isplenji, southern Baluchistan, and Las Bela; Henry Field in Makran and Bhawalpur; A. Ghosh, B.B. Lal, and B.K. Thapar in Bikaner and northern India; and S.R. Rao in Gujarat. Related work by V.M. Masson in Turkmenistan also has a bearing.

Five major stages of development are emerging. Many details are missing, but the broad outline can be summarized with reasonable accuracy. It bears directly on the origin of the Harappan civilization and is somewhat surprising, since it suggests a somewhat unexpected degree of independent development of that civilization.

STAGE I: PASTORALISM WITH LIMITED CULTIVATION
Figure 6.2

The evidence for Stage I, as yet poorly known, indicates that a people having domesticated cattle, sheep, and goats and a limited cereal agriculture were settled in parts of the Indo-Iranian Borderlands. They possessed a flake-blade, stone-tool industry which included sickle blades, gouges, and arrowheads. They also used ground-stone implements. Bone needles or awls are attested. Handmade pottery, often basket-marked and in some cases crudely painted, appeared in this stage, especially in its later manifestations. Reed matting was used as a floor covering. Small houses constructed of *pisé* or crude bricks produce a sense of impermanence to this stage, suggesting that considerable seasonal movement was the practice. Hearths were inside the houses. The most recent dates for this stage are around 3300 B.C., but it may extend back well into the fifth millennium before Christ.

The stage is represented in southern Afghanistan (Mundigak Period I, early phases), and, in northern Baluchistan, at Quetta (Kili Gul Mohammad I-II), Loralai (Rana Ghundai Ia, Sur Jangal I, Dabar Kot), and Zhob (Periano Ghundai), and in the northern part of southern Kalat (Anjira I). This distribution suggests that the stage was concentrated in the northern region. However, flake-blade tool industries are known in southern Baluchistan (Kapoto, Wadh, Ornach) and in southwestern Sind up to the Karachi area (Jherruck, Lyari, Tharro Hill, Karachi golf course). Among these, occasional geometrics indicate a possible link to earlier "Mesolithic" food-collecting industries which

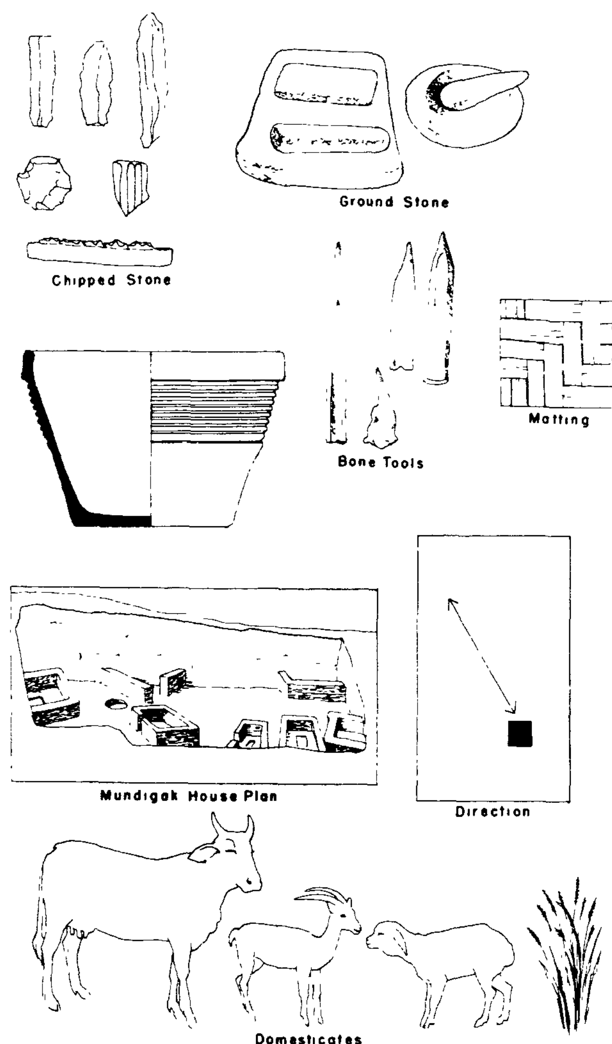


FIGURE 6.2. *Stage I pastoralism with limited cultivation. The rectangle outlines direction or the movement to and from a typical village. Pastoralism and cultivation are given equal value, with some emphasis toward the former. Villages are occupied seasonally*

are still poorly known in the subcontinent. Accordingly this stage was very likely much more widely distributed than our present evidence indicates. A detailed account of the correlations and stratigraphic relationships of the various sites of the Indo-Iranian Borderlands for this stage and later stages will be published in the near future.

The tradition bears a relationship to the Sialk I-Djeitun level of development of Iran and Turkmenistan, respectively, but is so much later, at least according to present evidence, that it appears to be a localized and long-enduring survival of that tradition. The Indo-Iranian Borderland ceramic correlations with Iran proper, for this stage and the following stages, were discussed in Bacon (1963: 265-278, and especially Figs. 5-9).

STAGE II: DEVELOPED CULTIVATION AND PASTORALISM: BEGINNINGS OF REGIONALIZATION
Figure 6.3

In this stage, the dwellings were larger and more substantial than those of the previous stage. Mud brick was fully used as a building material, as well as boulders and cut stone. Permanent village life is indicated. Stone dams, both terrace and, apparently, storage, were first employed in this stage. Copper was widely used. Ceramics were both handmade and wheelmade. Characteristic wares include red-on-black open bowls, white-on-black open bowls, and pedestal cups. Among the painted motifs on pottery are rows of ibexes and humpless and humped bulls, as well as a vast repertoire of geometric designs, most of which can be found in an earlier prehistoric context in Iran (especially in the north and northeast). Burials in or among houses were complete with funerary equipment. Potters' marks occurred, especially on the finer vessels. Goats, sheep, and cattle were herded probably much as in modern times, with seasonal movement.

In the earlier phases of this stage there seems to have been a general distribution of the same assemblage of artifacts from southern Afghanistan to central Baluchistan. However, in its later phases, distinct regionalization occurred, superficially represented in the pottery by the canister vessels decorated in polychrome or with concentric designs of the so-called Nal type prevalent in central Baluchistan and extending as far south as Kolwa and Las Bela. The fine-line Kechi Beg wares of the Quetta Valley and their equivalents in Loralai and Zhob represent another regionalization. Burial within the houses occurred in central Baluchistan (Nal) but was absent from Quetta and southern Afghanistan. This regionalization, it must be emphasized, did not change the essential techno-economic level shared throughout the Borderlands, though individual development was most probably uneven.

The stage dates from about 3300 B.C. to about 2500 B.C. Its earlier aspect is best represented in southern Afghanistan (Mundigak Period I, late phases, and Mundigak Period II); in Baluchistan at Quetta (Kili Gul Mohammad III-IV, Damb

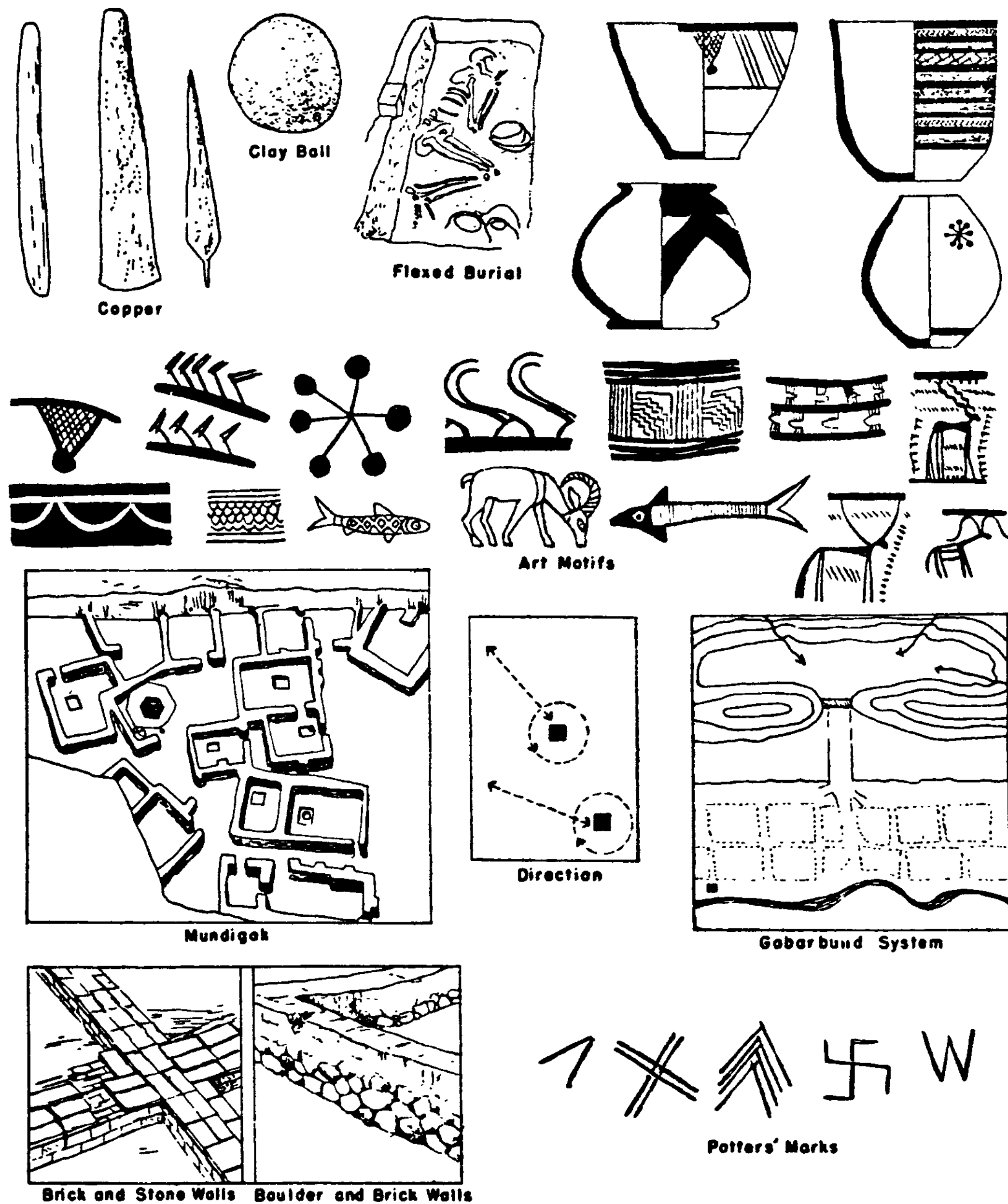


FIGURE 6.3. *Stage II, developed cultivation with pastoralism; the beginnings of regionalization. Direction indicates permanent occupation of villages, with limited pastoralism.*

Sadaat I), Loralai (Rana Ghundai Ib-II and Sur Jangal I-II, Dabar Kot), Zhob (Periano Ghundai, Moghul Ghundai); and in central Baluchistan (Anjira II-III). Its more recent aspects are at Nal (Surkh Damb) and, in southern Baluchistan, at Drakalo, Kolwa, Ornach, Wadh, and Las Bela. The latest phases also have been reported as having been found up to the borders of the Indus River Valley proper, in the Bolan Pass, in the upper Hab River Valley (near Diwana), and along the Khirtar mountain slopes facing Lake Manchhar as far north as Kachhi (Pandi Wahi, Ghazi Shah, Pai-jo-kotiro).

This stage reflects influences from northern Iran, especially the Chasmi Ali horizon in its earlier phases and Hissar I-IIA, Sialk III (earlier phases), in its later phases.

Stage III: Fully Developed Sedentary Village Life: Regionalization but Interregional Contact
Figure 6.4

This stage represents the occurrence of the largest number of villages for almost every region of the Borderlands. The villages as a whole achieved their maximum size. Accordingly, one may suspect with reason, this was the time of maximum use of the cultivable land. A wide variety of copper and bronze pins, knives, and axes (including socketed types) is represented. Human female and cattle figurines in terracotta were very common. Also common were compartmented seals, an elaborate system of potters' marks, alabaster vessels, and an extraordinary range of pottery forms, including pedestal vessels, plates (especially in grayware), and open-based immovable bread ovens set in the houses. The painted designs were often broad-line variants of motifs known in Stage II. There was also much emphasis on the elaboration of simple geometric forms, as, for example, in the so-called Quetta ware.

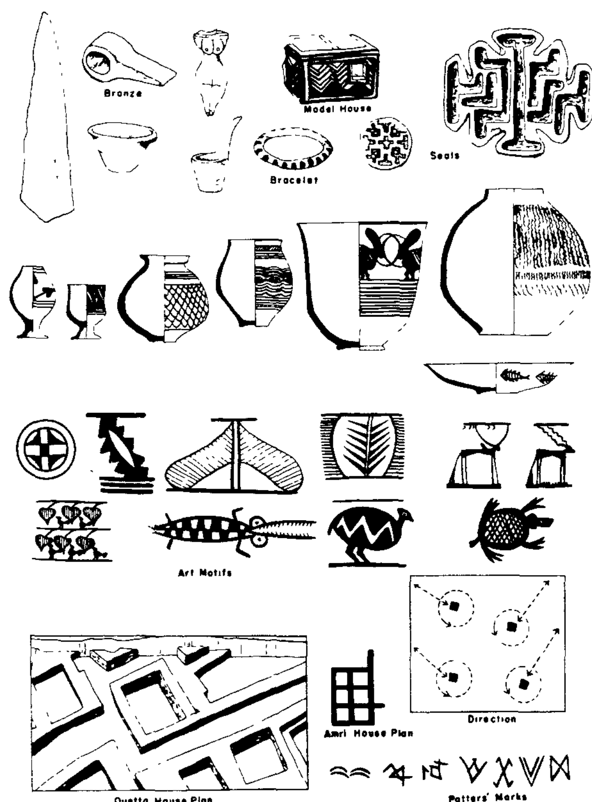


FIGURE 6.4. *Stage III, fully developed sedentary village life; regionalization continued, but there was considerable interregional contact. Direction demonstrates multiplicity of villages.*

Regionalization is very apparent in this stage. From the known material it is obvious that, although the generic obligation is distinctively to northern Iran in particular, the localization of village life and the influence of a different ecology had their effect. Now we find pipal leaves, humped Brahma bulls, cobras, birds, and fish depicted on the pottery. Remarkable are the rows of humped cattle which stand in the same stylistic fashion as the Iranian ibex of the previous stage. These emphases are superficial examples of possible qualitative changes in religion and social structure. These changes can be understood as representing a process of Indianization fully under way. In effect, the styles that brought the cultural forms into the characteristic subcontinental framework were coming into focus.

In this stage there is ample evidence that the farmers were moving out into the alluvial plain of the Indus River Valley. The Bolan, Mula, Baran Nai, Gaj, and other passes or related valleys, where village sites of Stage III have been found, provide excellent evidence of movement off the Iranian plateau. The lower levels of the site of Amri demonstrate not only the presence of settlers in the Indus Valley, but that they were already adapted to the new ecological situation. The recent exacavations at that site uncovered evidence for the existence of buildings that were partitioned into small doorless compartments, which suggest aboveground superstructures—an effective living arrangement in a land of floods and heat. At Kot Diji in northern Sind across the Indus River, F.A. Khan unearthed a massive fortification of the period, one somewhat duplicated at Tharro Hill in southern Sind. Sizable settlements of this stage have been discovered as far east as northern Rajasthan by archeologists of the Archaeological Survey of India.

That the farmers of the day were capable of dealing with the demands of the land on which they lived is best proved by the presence of massive stone dams or *gabarbands* adjacent to their villages, particularly in southern Baluchistan and southwestern Sind. These structures are of two types, storage or reservoir, and more frequently *kach* or terrace dams like those of south Arabia or the Negeb (Fig. 6.5). A discussion of the *gabarbands* of Baluchistan can be found in Dales (1962a:30-39), Fairservis (1961f), and Raikes and Dyson (1961:265-281); see also Evenari *et al.* (1961: 979-996).

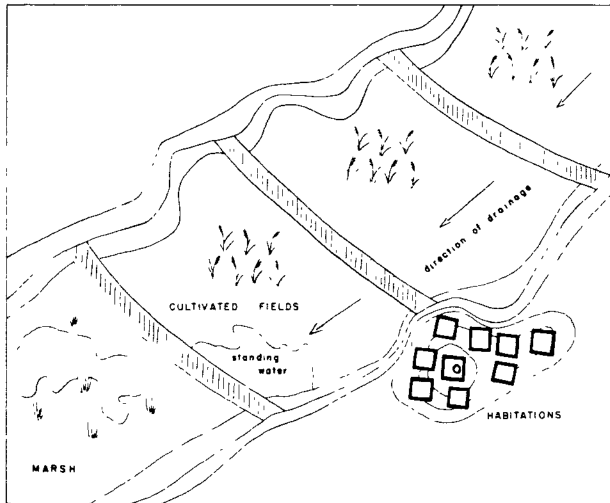


FIGURE 6.5. *Idealized drawing of a* kach *system as used in southwestern Sind in both ancient and modern times.*

Whereas these people share a range of artifacts that establish common generic ties and contemporary contacts, there are, nonetheless, stylistic differences proving that regionalization was maintained even within the Indus River Valley. Thus the rather austere, simple, geometric, painted designs of Kot Diji are in sharp contrast to the exuberant compositions found at Amri. Most significant is the style found in northern Sind, along the southern borders of the plain of Kachhi, and the foothills of the Khirtar Range. Apparently a localized outgrowth of the Kulli-Quetta culture styles of Baluchistan, it is characterized by such features as unpainted terra-cotta bangles round in cross section, female and animal figurines with rather gross features, so-called "cakes" of terra cotta, and a pottery corpus closely related to that of Baluchistan. However, the painted designs on the pottery, though derived from Baluchistan, are characteristically composed in overall patterns in which floral elements have a special place. Unfortunately, no excavator has as yet uncovered these settlements, but surface remains indicate that the villages were probably large and flourishing. The above style characterizes some of the styles of the Harappan civilization and, in fact, is known to have existed prior to the mature phase of that civilization at certain sites, including Mohenjo-daro. This provides a reason to label it Early Harappan and thus in the direct line to the so-called mature or urbanized phase of that culture.

On the present evidence this stage can be dated from about 2500 B.C. to perhaps 2300-2200 B.C. It is widely represented in the Borderlands: southern Afghanistan (Mundigak III, Deh Morasi Ghundai); in Baluchistan: Quetta (Damb Sadaat II), Loralai (Dabar Kot, Sur Jangal III, Rana Ghundai III), Zhob (Periano Ghundai, Moghul Ghundai); in central Baluchistan (Anjira IV and surface): in Kolwa (Kulli, Mehi), Nal, Ornach, Wadh, Drakalo, Las Bela (Edith Shahr, Niai Buthi); in southwestern Sind (Amri, Kohtras Buthi, Tharro Hill), in the Lake Manchar region (Pandi Wahi, Ghazi Shah), in northern Sind and Kachhi (Gandava, Judeirjo-daro, Mohenjo-daro, Kot Diji), in Bhahawalpur (Bhut), and in Bikaner (Sothi, Kalibangan, and other sites).

It seems clear that the strong influx of northern Iranian influence continued, since the later phases of Sialk III and materials from Turkmenistan (Namazgah Tepe III) have rather precise equivalents in Baluchistan (Bacon 1963:271, Fig. 78; Masson 1960:31; Piggott 1943). However, for the first time it appears that influences from southern Iran have entered the picture. The principal occupation of the Seistan basin of the Helmand River (Fairservis 1961a) and the basins southward (Bampur, Parom) to the Arabian seacoast took place at this time. The southern Iranian cultural style, as nearly as we can now identify it, can be found in western Makran. Some aspects are known as far as Kolwa to the east.

STAGE IV: THE PERIOD OF URBANIZATION
Figures 6.6 and 6.7

The characteristic feature of this period of urbanization was a monumental building situated on the highest part of a mound or other elevation, natural or artificial. Usually it was surrounded by a wall, and a well or drain at the highest point of the structure was a feature. In some cases the buildings were multiple structures. On the lower slopes of the mound or in the surrounding area, or both, formal structures such as baths or rooms set in rows were situated. At Mundigak in Afghanistan an elaborate pillared facade more than 20 meters long occurred in front of a monumental building set high on the site. At Damb Sadaat in the Quetta Valley a platform, with drains, was situated at the top of the site. A disarticulated human skull was found in a cache in one corner of its foundation. In Las Bela, Complex A of the site group Edith Shahr represents an almost continuous complex of stone structures along the Porali Valley above the fertile zone of Welpat. These massive structures dominate the

Bronze
Extended Burial
Copper
Wheeled Cart
Weights
Figurines
Direction
Art Motifs
Seal Motifs

FIGURE 6.6. *Stage IV, the period of urbanization. Direction emphasizes movement from villages to urban centers for special activities and return. Limited pastoralism.*

narrow valley north of the cultivated plain. Such large structures are known in Kolwa, Drakalo, and the Ornach, as well as in Zhob and Loralai.

Pedestal figurines of goggle-eyed women with prominent breasts and wearing heavy necklaces, and painted figurines representing cattle, are typical of the period. The present evidence suggests burial in cemeteries. Pottery tends to be heavy. Though painted motifs characteristic of the earlier period occur, they tend to be exceedingly stylized. In the Late Kulli pottery (in southern Baluchistan principally) scenes are painted, and the typical horizontality of the decoration of the earlier wares is less obvious. As in the earlier stage, clay bangles, beads in lapis lazuli, carnelian, agate, serpentine, steatite, and other stones, clay and steatite seals, copper and bronze implements, alabaster vessels, ground stone, flake-blade tools, and bone and ivory ornaments were common. The weaving of bast fibers and perhaps cotton is indicated.

The settlement patterns of the time suggest that there were areas such as northern Las Bela where complexes of monumental structures were separated from the farmers' villages. The latter were in the midst of the cultivable area. In Kolwa, southern Kalat, Zhob, Loralai, southwestern Sind, and Quetta, monumental structures occurred in the midst of the villages, either on a promontory within the village or immediate beyond its boundaries.

In the Indus River Valley the massive urban developments at Mohenjo-daro, Judeirjo-daro, Chanhudaro, and, later, at Harappa manifestly belong in the same developmental line as that found in eastern Baluchistan and southern Afghanistan. Characteristically, the highest part of a typical mature Harappan site, even including village sites, is marked by monumental structures which possess drains and wells (?) as integral parts. These structures are often surrounded by walls. At Mohenjo-daro a colonnaded bath, rooms with drains in their floors, and a probable storage area or granary were within these walls.

Most impressive is the extent of the main Harappan sites as exemplified by Mohenjo-daro. The habitation areas east of the "citadel" cover 1,500,000 square feet, and the population could have numbered about 40,000. Comparable population estimates can be found in Adams (1962:6-7), Braidwood and Reed (1957:19-31), and Frankfort (1948). The vast number of objects in many materials recovered in the excavations demonstrate the full-time activities of such professions as the metal smith, potter, weaver, seal carver, brick maker, figurine maker, and even possibly the toy manufacturer.

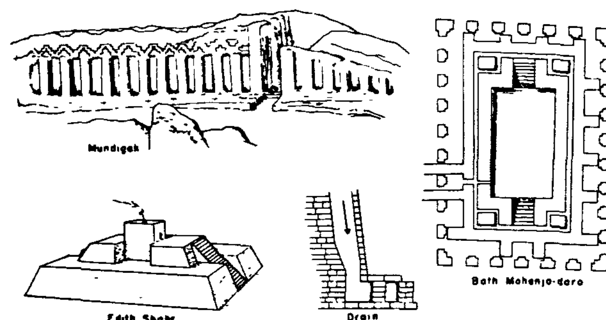


FIGURE 6.7. *Characteristic buildings of Stage IV.*

Direct contact with Mesopotamia may also have been a not uncommon occurrence in this period. Harappan seals have been found in late Akkadian context in Mesopotamia (Wheeler 1953:85; Gadd 1932). The technique of inlay, certain bead forms, button seals, and some decorative elements such as the trefoil pattern and possibly the Gilgamesh (?) motif bear witness to Mesopotamian contacts. The Harappan wheeled cart, and some of the more utilitarian traits such as weights and measures, may have their roots in the stimulus of Mesopotamia. The enigmatic Harappan script found on the square seals of the time may owe at least part of its motivation to Mesopotamia, though the potters' marks of the earlier stages had a distinct role in the development of the script (Fairservis 1956:328-329).

The Harappan civilization can be said to have achieved its characteristic style indigenously; its elaboration may be the result of Mesopotamian contact. However, it cannot be said that its origin is in any way divorced from the obvious line of development in Baluchistan and Afghanistan. On the present evidence it seems reasonable to assume that the Harappan civilization stemmed from the developing village complex characteristic of much of Iran in the third millennium before Christ. Apparently economic advantages inherent in the Indus Valley situation motivated the production of surpluses, the proliferation of populations, the amplification and the multiplication of non-farming specialists, and, in turn, the improvement or elaboration of traits already possessed or received by that population. (However, the pitfalls encountered in too close adherence to economic determinism were discussed by Adams 1960b.)

On the present evidence we can assign dates of from 2300 B.C. to 1700 B.C. for the stage in the Indo-Iranian

Borderlands. It is best represented in southern Afghanistan (Mundigak IV), and in Baluchistan at Quetta (Damb Sadaat III), Loralai (Dabar Kot, Rana Ghundai III-IV), and Zhob (Periano-Ghundai-Zhob Cult), and all the so-called Kulli-Harappan levels in sites of southern Baluchistan (Nindowari, Kulli, Mehi, Edith Shahr). Harappan sites of this period are found in some number in southwestern Sind (Kohtras Buthi, Karchat, three sites in Malir, and other areas), and three are known along the Arabian seacoast (Bala Kot, Sotka Koh, and Sutkagen-dor). Mohenjo-daro, Chanhu-daro, the Gandava sites, Judeirjo-daro, Kot-Diji, and Lohumjo-daro are other well-known sites in Sind and Kachhi. At Dabar Kot in Loralai District a Harappan level can be seen high on the mound. Mature Harappan sites are far-flung, in India reaching into the eastern Punjab, Gujarat, and Rajasthan. Their distribution was well attested by Rao (1963a:5-27) and Wheeler (1959:95).

Stage V: Economic Decline and the General Abandonment of the Indo-Iranian Borderlands by Farmers of Developed Villages

Throughout Baluchistan only the largest village sites continued to be inhabited. The bulk of the villages were abandoned. Such occupation remains as have been found reflect a decline in the quality of craftsmanship and a degradation of building practices. In the Indus River Valley generally, only the largest sites continued to be occupied, and in an increasingly deteriorating situation. J.M. Casal (1964b) has suggested that Jhukar is a late Harappan manifestation which, in view of the character and distribution of the Jhukar assemblage, agrees with these evidences. However, flourishing late Harappan manifestations are found in the Punjab and in Gujarat, indicating that the populations of Sind and Baluchistan moved eastward, abandoning their old habitat. The universal aspect of this abandonment, uneven in accomplishment, suggests general economic decline, for if the optimum living conditions of the previous stage could have continued, some sites would bear evidence of this situation, and, if hypothetical or actual invaders had merely taken over a prosperous area, it would hardly have been generally abandoned.

In the latter part of the period, apparently, new people and cultures did arrive on the scene, some aggressively, but as a whole the evidence suggests that only a few of these people came into conflict with an established generation of older occupants. (The problem of the post-Harappan occupation of the Indo-Iranian Borderlands has been discussed by De Cardi 1951:63-75; see also Fairservis 1961f).

The period can be dated after 1700 B.C., and it probably lasted as late as 1200 B.C. or even 800 B.C. in some regions of the Borderlands.

CHARACTERISTICS OF INDO-IRANIAN BORDERLAND DEVELOPMENT

Certain points brought out by the above outline must be emphasized:

(1) Once started, there was a general continuity of occupation. The stages generally overlapped and regional advances were uneven.

In this regard several individuals have emphasized their objections to the seriation technique that we used in our excavations in Quetta and Loralai (Piggott 1962:254; Dales 1966b:160ff.). These objections stem from a misunderstanding of the reasons for our using the technique. Seriation, as a technique, cannot be defended within the scope of the present paper. Its validity as a tool in archeology has been discussed and confirmed. One can find examples of it in the publications of many archeologists, of whom Kroeber, Nelson, Ford, Willey, and Spaulding are good examples. Until methodologies are used in subcontinental archeology that recognize the dynamics of culture history rather than history *per se*, we will be playing at the philately, which R.E.M. Wheeler criticized years ago. If ceramics (among other things) are to be used as correlative evidence, we had better know what happens to those ceramics where we excavate, before we establish chronological schemes which use them to tie Baluch or Sindi prehistoric villages typologically to Susa, Samarkand, or Timbuctoo! This means that all the ceramic evidence must be considered. The evidence must not be confined to whole vessel, painted wares, and other *object d'art* alone. Correspondingly, the overuse of floor levels, walls, gates, hearths, and other architectural features as evidence of catastrophe, excellence, hierarchy, political event, or, indeed, the bounds of chronology, or cultural style, is now retarding subcontinental archeology as much as did sea-level stratigraphy in pre-Wheeler days. The evidence for human habitation in these prehistoric periods demonstrates both continuity and change. Cultural continuity and cultural change are not nor-

mally the accidents of environment but represent the process of man's societal and subsistence-seeking activities. Until archeologists are willing to study and to attempt to understand the complexities of the cultural phenomena that anthropology reviews, we will continue to have "seven-league-boot" schemes as remote from the actual situation as Velikovsky is from Darwin. Cultural change is not simply a matter of fallen walls and the appearance of polychrome pottery; it is a qualitative matter affecting the whole society. On this basis it behooves us to make our methodologies as sensitive as possible by some conscientious effort to utilize all the instrumentation available to us. Seriation is one method that has been tried and proved to be of use. It needs to be more thoroughly understood and put to more widespread use in subcontinental archeology. It is only one tool among many, but it is a good tool. Merely finding

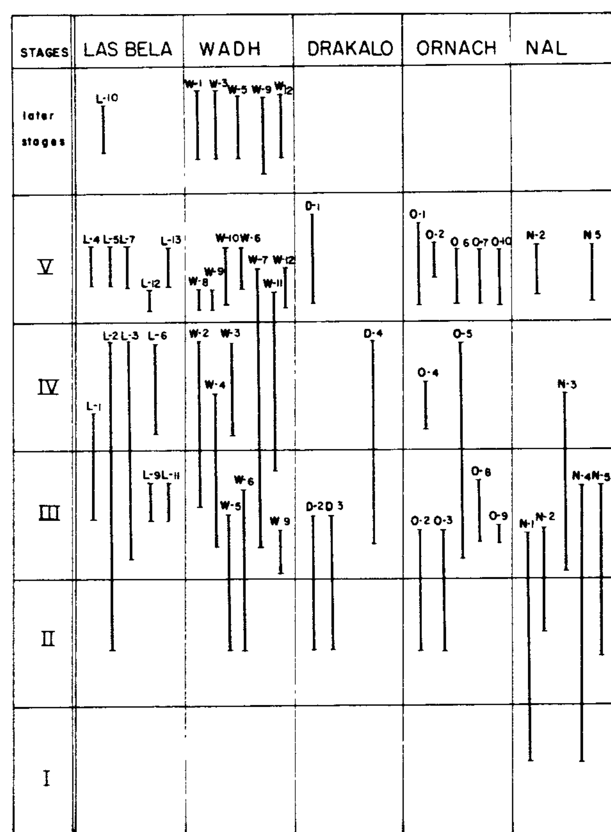


FIGURE 6.8. *Regional representation of late prehistoric sites of southern Baluchistan arranged according to stages represented generally by surface collections. Later stages are incomplete. Regions are arranged from south* (left) *to north* (right) *Note the apparent abandonment of these regions between Stages IV and V.*

material in stratigraphic context and describing it are not enough. We need especially to assess our methodologies when at least 90 per cent of all habitation sites are villages and camp grounds. Techniques for excavating cities may not always be viable under these circumstances. Our archeological strategies should be accompanied by a little wisdom and much care.

Concomitantly, our grounds for postulating vast chronological schemes that tie the Borderlands to western Asia typologically are much too weak to bear all the emphases researchers place on them. We are hardly able to trace the ties between Sind and Baluchistan on our present evidence.

(2) The cultural conformity in Stages I and II was widespread. Conformity changed to regionalization in the later phases of Stage II and in Stage III. In Stage IV, while regionalization persisted in some regions, particularly in northern Baluchistan and Afghanistan, Sind, Punjab, Gujarat, northern Rajasthan, and southern Baluchistan were all settings for the same cultural style—the Harappan.

(3) The links to Iran, especially to the north, were very strong from Stages I to III. However, the Indo-Iranian Borderlands were remote, and trait diffusion was slow in Stages I and II. By Stage III, however, diffusion was accelerated. In Stage IV we have evidence for direct contact with the west over land or by water. However, in both Baluchistan and in Sind (Figs. 6.1 and 6.8), the line of development was from north to south. This emphasizes the probable role of northern Iran and Turkmenistan as the sources of some of the principal cultural forms of the Indo-Iranian Borderlands.

(4) Critical to an understanding of the processes that created the Harappan civilization is recognition of the fact that it is subcontinental in character. Neither the developmental motivation provided by Iran nor the probable technological advantages gained through contact with Mesopotamia changed its essential style.

The evidence indicates that the urban situation in the Indus River Valley was a logical development from advanced village farming in an optimum situation. In a complex of traits for which we have good evidence, India is prominent: multifaced deities, the "yoga" position, sacred cobras, phallic worship, cattle cults, ritual bathing, numerous bangles as female ornaments, cattle painting, motifs based on local fauna and flora, and horned headdresses all can be said to be subcontinental traits of the Harappan civilization. (However, some of these so-called "Indian" traits have been

disputed by Sullivan 1964:115-125.) It does not require much stretch of the imagination to recognize the roots of village India here. What is absent, of course, is the tangible body of societal and familial evidences that we need to recreate the specific nature of that village-urban society.

(5) The final eclipse of the Harappan cultural style may have occurred when the knowledge of rice cultivation opened more southerly and easterly regions to the cereal farmers. One form of rice has been identified at the Harappan site of Lothal in Gujarat. Although it appears to have been undomesticated, its presence suggests that a stage of incipient rice cultivation was under way by at least the middle of the second millennium before Christ.

(6) The chronological scale suggests that in less than 1500 years the former settlers of the Borderlands attained urbanization—a rapid rate of development that indicates the effect of contact with Iran and the west and the vigor of indigenous development.

CHARACTER OF THE HARAPPAN CIVILIZATION

As now known, Harappan civilization possesses a characteristic style or form. Wherever it appears it presents a particularly striking unity; certain traits are virtually identical at sites distributed from the Arabian seacoast to the foothills of the Himalayas. These traits include location close to the active flood plain of a river, brick buildings on platforms with drains, toy carts, terra-cotta "cakes," clay bangles, bone or ivory inlay, female and animal figurines, seals and seal writing (identical in motif and sign), copper tools and weapons, weights, clay balls or pellets, the pottery corpus, graters, bathtubs, and an emphasis on representation of cattle on seals and figurines. At widely dispersed locations such as Lothal (Gujarat), Kalibangan (Bikaner), Dabar Kot (northern Baluchistan), Sutkagen-dor (Makran), Harappa (Punjab), and Mohenjo-daro (Sind) monumental structures are situated on the highest parts of the sites. This practice is widely duplicated in Baluchistan to the west in related sites. The overall impression is that of a stable conservative civilization reminiscent of Old Kingdom Egypt, but certainly differing from the impression one has of ancient Sumeria. Some authorities have assumed that this conservatism was largely the result of a political administration, probably theocratic, whose rule is best exemplified by the fact that monumental structures were situated within the walls of a "citadel," i.e., the elevated portion characteristically found on the sites. Thus, the two largest sites, Harappa and Mohenjo-daro, were twin administrative capitals. Some emphasis has been placed on the identification of these structures as temple storehouses, granaries, barracks, and the like, which underlines the autocratic nature of administrative control (Piggott 1950). The overall description is apparently based on a socio-economic determinist pattern, of which the late V. Gordon Childe was a leading exponent. It is clearly modeled on Mesopotamian parallels.

However, if we examine these parallels, the differences are readily apparent. Sumerian civilization is characterized by a multiplicity of cities, an increasingly efficient writing system, a peculiar man-god relationship, a progressive technology, a changing art style, a variety of international contacts, speculative thought, organized warfare, an extensive and effective irrigation system of characteristic form, and a concentration within a limited geographical area ecologically undifferentiated. These traits and trait complexes are largely absent from the Harappan civilization, according to the evidence we now have at hand.

The chronological priority of Sumerian civilization among world civilizations and the general role now accredited to Mesopotamian civilization in motivating the pre-dynastic florescence that preceded Egyptian civilization have naturally directed scholars to assume that Indian civilization stemmed from direct contact with Sumer or that pre-civilized Indians were motivated by stimulus from Mesopotamia. As Sir Mortimer Wheeler, the leading interpreter of the Harappan civilization, wrote, "the idea of civilization was in the air" (Wheeler 1959:104). This Mesopotamian ethnocentricism has been reinforced by the discovery of Harappan seals in Mesopotamia confirmed in Akkadian context or even earlier. Cylinder seals have also been found in Harappan context (Gadd 1932; Mackay 1938b:344, Vol. 2:pl. 89, No. 376 and D, pl. 96, no. 488). The Sumerian reference to the populous land of Dilmun to the east has motivated the eminent Sumerologist, S.N. Kramer, justifiably to postulate the possibility that the allusion was to the Indus River Valley (Kramer, 1963a:281-4; 1964: 44-52). However, he also saw the possibility that people of the Ubaid period, driven from Mesopotamia by the Sumerians, came to the Indus Valley where they initiated the development of civilization. As pointed out above, the Harappan style was already flourishing

by Stage III. The Harappan civilization of Stage IV is a direct development from that stage. Nothing in the evidence suggests that the "urbanization" within the Indo-Iranian Borderlands of Stage IV was the result of exterior influences. The special florescence of culture at Mohenjo-daro and other "urban" sites apparently had its source in a built-in advantage based on the technology and interacting population of an already developed farming culture and rendered effective by the advantages of the Indus River ecology. There is little doubt that had soil, water, and a more concentrated population been possible, urban situations could have developed contemporaneously in southern Afghanistan and in parts of Baluchistan.

As a theoretical scheme, it is suggested that the contact between the highlands and the Indus plain had a significant role in developing the Harappan civilization. Given the concentration of settlement made possible within the confines of the fertile but limited expanses of the neighboring upland valleys of the Indo-Iranian Borderlands, human interaction accelerated and ideas moved across regional boundaries. Initially, this interaction was maintained when the Indus River Valley was settled. As time passed in the almost unrestricted expanse of the Sind-Punjab-western India area, concentration of population was reduced, interaction between differing societies slowed, and, unstimulated to major change, men settled into stable repetitive lives. Thus, the characteristic style of the Harappan civilization was created.

But, in point of fact, we are investigating a civilization, the written records of which are still untranslated and of which the archeology is largely limited to excavation at the larger more urban sites. These facts have undeniably tended to color interpretations of the Harappan civilization, as if, for example, one visited London or New York and used these cities to represent either the whole of Great Britain or the United States. More than 150 sites of the Harappan civilization are known; of these, only three or four can qualify as "urban."

In view of the speculative quality of the interpretations of the Harappan civilization, the recent appearance of new and important evidence bearing on them is significant.

The first body of new evidence arises from the increase of excavated remains of pre-Harappan times. Prior to 1955 the limited excavations of Majumdar carried out in the early 1930s, and Sir Mortimer Wheeler's discovery of a non-Harappan level at Harappa, were the only sources for evidence of pre-Harappan occupation of the Indus Valley. In 1955 F.A. Khan at Kot Diji in Upper Sind revealed an extensive pre-Harappan settlement which contained abundant evidence bearing on the indigenous developmental character of the Harappan civilization. Somewhat later, J.M. Casal explored the site of Amri in southwestern Sind which had been previously only superficially excavated by Majumdar. Here the evidence points to rather close typological connections of pre-Harappan Amri with Pre-Harappan Kot Diji and with comparable village assemblages of southern Baluchistan. At Amri the developmental line is less clear. One gains the impression that in its mature form the Harappan civilization was superimposed from without. This impression is reinforced by a similar phenomenon occurring in small village sites found by Majumdar in the vicinity of Lake Manchhar some 40 miles to the north of Amri. The Archaeological Survey of India, particularly on the basis of the survey by A. Ghosh (1965b) and the excavations of B.B. Lal and B.K. Thapar, recorded an extensive pre-Harappan settlement along the banks of the now extinct Ghaggar River branch of the Indus system in Bikaner in northern Rajasthan. Their excavations at Kalibangan suggest the existence of ties to pre-Harappan Kot Diji. As at Amri, the major Harappan occupation is of the so-called mature type. The unexcavated site of Bhut in Bhawalpur marks the existence of pre-mature Harappan settlement between Bikaner and the type site. Beatrice de Cardi (1964:20-29) has found an extensive site at Gandava at the entrance to the Mula Pass into Baluchistan, and Raikes has discovered the not-too-distant site of Judeirjo-daro near Jacobadad on the flat alluvial plain of Kachhi (Casal 1964b: 11-12; 1964a). The Gandava sites and Judeirjo-daro bear surface evidence attributable to material of Kot Dijian type and for a range of artifacts that we can now classify as Early Harappan—in other words, precisely that developmental stage which has been missing from the record. The stage is now recognized only categorically in terms of changes in pottery form and decoration, figurine type, the presence of certain characteristic Harappan objects such as toy carts (in prototypical form) and terra-cotta "cakes," and the mutual dependence upon the same resources in soil and water. For Baluchistan the evidence indicates that the south, i.e., southern Kalat, Las Bela, and Makran were the last areas settled, whereas the north, i.e., northern Kalat, Quetta-Pishin, Kachhi, Zhob, and Loralai,

provides the earliest evidence of settlement within Pakistan Baluchistan. (These researches will be published in a later study.) This north-to-south trend appears in the Indus Valley, especially in Sind, where, on the basis of the present evidence, the Early Harappan and Kot Dijian settlements occurred in the north, whereas the mature Harappan settlements are found as far south as the Arabian seacoast and the Kohistan portion of southwestern Sind and the Malir River drainage (Fig. 6.1).

The cultural ties of the Indus Valley to the Baluchistan sequence are especially important, because there is evidence in Baluchistan for an early development of elaborate irrigation systems of the *kach* or bund type (Dales 1962a; Fairservis 1961f; Raikes and Dyson 1961; Stein 1931) for the development of monumental structures using drains, and for a system of mutually intelligible symbols that can be regarded as one source, at least of the form, of the Harappan script (Fairservis 1956a:328-329). There is good evidence for the occurrence of an Amri storage dam and for the contemporary use of stone-reflecting or terrace dams in southern Baluchistan. Dams were used at a number of Harappan sites; those at Kohtras Buthi and perhaps the Malir River are especially notable. These features of pre-Harappan Baluchistan have not as yet been reported in the Indus Valley, but the gross size of sites such as Judeirjo-daro and those of Gandava as well as what is known of the earliest stages at Mohenjo-daro all goes beyond suggesting that regional developmental advances in the Indo-Iranian Borderlands were mutually influential. In fact, it is very clear that in Baluchistan at least a process we have termed "Indianization" was under way. This process reached its climax in the development of large "ceremonial" structures that contained evidence of human and animal sacrifice (especially cattle), the ritual (?) use of water, and for the adoption of Harappan or Harappan-like objects for daily use (Fairservis 1959; 1961f).

These trends in the interpretation of the new evidence for the locale of the Early Harappan development and the dawn of civilization in the Indian subcontinent are obviously of great importance, but the present lack of full-fledged and purposeful excavation at the key sites makes it impossible to trace the details of these trends. What is important, however, is that on the present evidence we believe that the style or form of the Indus Valley civilization had its earliest manifestations in a region remote from the coast where contact with Sumerian civilization is assumed to have occurred. This is not to say that such contact may not have occurred overland, as it probably did later, but that it is unlikely that contact diffusion with Mesopotamia germinated the Harappan civilization. It does suggest that the Indus Valley civilization was an indigenous development that arose out of the evolution of developed village cultures in a favorable environment. It emphasizes the subcontinental roots and the consequent "style" which gives the civilization its uniqueness.

A second body of more recent evidence apparently amplifies the above suggestions and helps to define the peculiar qualities of the mature Harappan civilization. The Indus River system and its relationships to human activity have been rather poorly known in spite of numerous studies that ranged over a century. A recent publication by Revelle (1964) is paramount among a number of significant studies bearing upon the man-river relationship in its modern terms (Government of Pakistan 1960; 1961). The fact that traditional methods of farming are the rule rather than the exception in much of modern Sind renders the Revelle report and similar ones of more than a little importance to the study of the Harappan civilization, particularly in view of the almost exact parallels that can be found between some artifacts of the Harappan civilization and those of rural twentieth century Sind. An approach to an understanding of aspects of the Harappan civilization might be made by using these modern data.

Mohenjo-daro, the largest of the Harappan sites, is close to the right bank of the modern Indus River in Larkana District of northern Sind. At the present time the surrounding area would be inundated annually were it not for a series of protective bunds, most of which are of recent origin. In modern times cultivation in the area has been wheat, millet (*jowar*) and rice—the last a rather recent introduction. The Larkana Canal, which has its source in the Lloyd Barrage at Sukkur, is the major source of water for cultivation in the area. However, the surplus canal water of the Larkana District is apparently allowed to percolate to the fields in areas of poor drainage, causing waterlogging (Revelle 1964:61-62). This rise in the water table has been rather dramatic since the completion of the Lloyd Barrage. The climate of northern Sind is semi-arid (annual rainfall, less than 10 inches). Since the mean annual temperature is around 90° F., the evaporation of surface water is very rapid. The salt content of the water table is high, and, through leaching, large areas have become unfit for cultivation. Recently the

Government of Pakistan has forbidden the cultivation of rice in the area, and there are plans to lower the water table by means of tube wells.

The Indus River at the Lloyd Barrage at Sukkur has, according to Revelle (1964), twice the annual flow of the Nile and 10 times that of the Colorado River. The immense amount of silt it carries constantly raises its bed and creates levees as it meanders over the flat alluvial plain of Sind. From Sukkur, south to the delta, the fall is only about 9 inches per mile. Above Sukkur, at Kashmor, the river is some 60-80 feet higher than the town of Jacobabad 70 miles to the west (Fig. 6.9). As late as Moghul times, the annual summer floods often

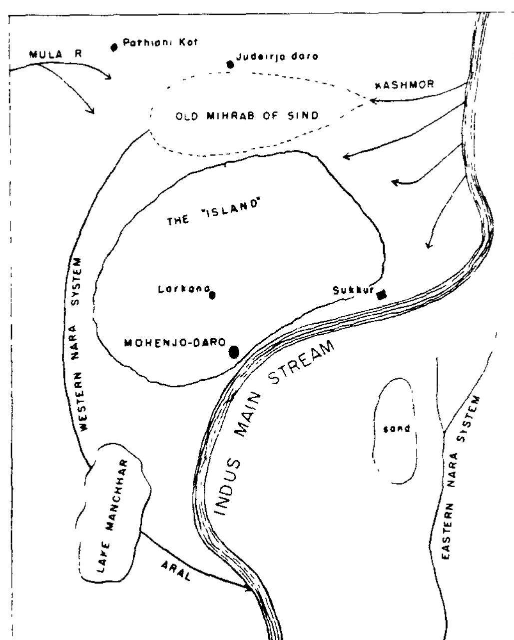


FIGURE 6.9. *Sketch map of the hydrographic system of the lower Indus Basin from Kashmor to the Aral outlet of Lake Manchhar*

caused breaks in the western levees above Sukkur (Heddle 1855:414-415; Raverty 1893). The flood waters would flow to the west, passing between Jacobabad in the north and Ratodero and Larkana on the south, and may have been the water resource for Judeirjo-daro. Eventually, these waters found an outlet in the so-called Western Nara System which runs parallel to the Indus River less than 40 miles to the west along the edge of the Khirtar Range of eastern Baluchistan whence some torrential water augmented the Nara flow. On the south, in the vicinity of the old Islamic city of Sehwan, the Nara waters pass into Lake Manchhar, a shallow, seasonally fluctuating body of water rarely deeper than 4 feet at the center of its basin. Passage from the lake to the south is blocked by the Laki Hills which extend eastward from the Khirtar. Thus the flood waters of Lake Manchhar find an outlet via the Aral Channel back to the main stream of the Indus. Though there is evidence that the general drift of the Indus River has been to the west since the late Pleistocene (Pithawalla 1959:80-81; Wadia 1957:392-393), it appears unlikely that the main features of the southern Indus Valley drainage have changed over the past 5000 years. The rock outcrops of Rohri near Sukkur tend to channel the main Indus drainage much as the cataracts affect the Nile River. To the east, the Thar Desert and the high ground around Khaipur have also played a role in determining the present position of the main Indus stream in northern Sind. However, some authorities have postulated the existence of a more easterly main stream marked by the bed of the now extinct Hakra Channel and the so-called Eastern Nara System (Pithawalla 1959:45-46; Lambrick 1964:33ff.). The Sarasvati-Ghaggar drainage, now extinct, would have augmented the eastern Indus Valley river system, however, and that, plus the possibility that flood waters moved eastward from Sukkur or more northerly regions, can account for the traces of river systems in the east. In any case, the present evidence for a sizable flow of river water in the Larkana-Lake Manchhar areas in the third millennium before Christ is substantial.

The distribution of Harappan sites in northern and western Sind emphasizes the accuracy of the concept that the ancient Indus system is generally comparable to the modern system (Fig. 6.1). The site of Judeirjo-daro, north of Jacobabad at the edge of the desolate plain of Kachhi, lies at the limit of the present-day, canal-irrigated cultivation. Lohumjo-daro is about 30 miles south of Mohenjo-daro. Jhukar is west of the modern city of Larkana, and Mohenjo-daro is 16 miles south of that city. Kot Diji and Naru Waro-dharo are at the edge of the Khairpur outcrops and gravel plain overlooking the left bank of the Indus. Two Harappan village sites are at the edge of the flood plain at Lake Manchhar. Amri, to the south, overlooks the Indus Valley from a position close to and even on the Kohistan gravel plain which forms the west rim of the valley in that region. For the rest, the Harappan sites of western Sind are geographically in Baluchistan; they are situated along the river and stream systems of the eastern rim of that mountainous region. In other

words, the bulk of the known Harappan sites in western and northern Sind are not part of the Indus Valley region at all. However, the Indus alluvium doubtless now conceals the smaller settlements, leaving only the larger (and higher) remains standing above the alluvium.

The existing remains in the Indus Valley itself demonstrate none of the characteristic settlement patterns had there been dependence on canal irrigation (other than short inundation canals). Rather, the individual locations strongly suggest that their *raison d'être* was access to the Indus flood plain for purposes of cultivation. The list of cultivated plants gathered by archeologists from remains found at Mohenjo-daro (Table 1) also bears witness to the practice of flood-plain agriculture, and strongly suggests that, on the present evidence, the farmers of Mohenjo-daro grew only a winter or *rabi* crop.

TABLE 1 FOOD CONTENT (PER 100 GRAMS) OF PRODUCTS IDENTIFIED FOR THE HARAPPAN CIVILIZATION

Foodstuffs	*Calorie Content*	*Protein Content in Per Cent*
Cereals (wheat and barley)	335	10.0
Vegetables		
Peas (*Pisum aesativum*)	339	23.8
Other		
Fruits		
Bananas	88	1.2
Figs	79	1.4
Mangos	66	0.7
Pomegranates	63	0.5
Melons	32	0 5
Dates		
Oils and fat		
Sesamun	568	19.3
Ghee	804	0 0
Sugar		
Honey (?)	300	1 3
Other (?)		
Meat and fish		
Beef	240	18 0
Mutton or goat	260	16 5
Fish	100	16 4
Milk and curds	80	4 2
Nuts (?)		

Four essential physical features are apparent in this Indus Valley alluvial area: the active flood plains, the meander flood plains, cover flood plains, and bars (Fig. 6.10). Of these features the bars and the cover

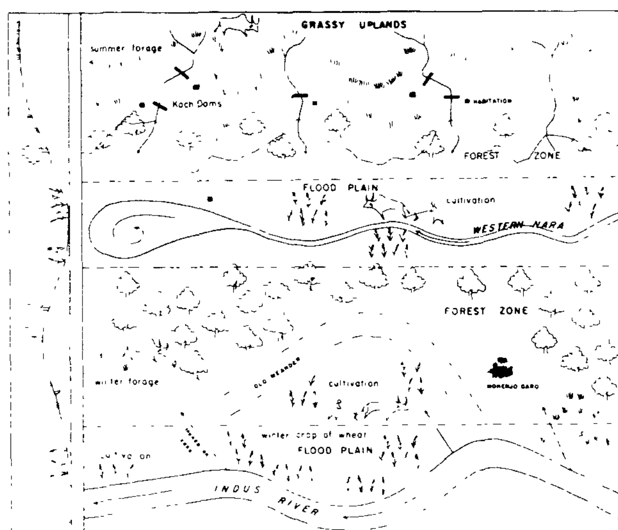


FIGURE 6.10. *Idealized cross section of the Indus River Valley along an east-west axis in the vicinity of Mohenjo daro. The captions relate to the ancient situation. Elevations, somewhat exaggerated, are on the left of the drawing.*

flood plains, the best for cultivation, amount to 12 million acres in Sind. These plains are now, of course, irrigated by canals. In Sind there are approximately 1.2 million acres of the active flood plains, some of which are cultivated today. The evidence for the Harappan civilization in Upper Sind suggests that neither the bars nor the cover flood plains were extensively cultivated.

Mohenjo-daro itself was not, of course, on the active flood plain, though it might be so today were it not for protective bunds. However, we can assume that it was situated close to the flood plain which was the center of farming activity—probably on the meander flood plain just as is the adjacent modern village of Hasan Wuhan.

The cover flood plain and to some extent the meander flood plain are outside and higher than the active flood zone, except in the circumstances of very high flooding. These areas are therefore the seat of forests which in Sind are essentially xerophytic and include acacia, capparis, zisyphus, and tamarix. However, the more watered areas have a more varied flora and include *Populus euphratica* (bahan), *Acacia arabica* (babul), *Prosopis spicigera* (kandi), *Dalbergia sissoo* (tali), and *Ficus*. *Acacia*, *Dalbergia*, *Prosopis*, and *Tamarix* sp. (lai) are all excellent sources of fuel (Ishaq, no date). The grassland, especially along the slopes of the valley rim and on the Baluch plateau and persisting in the higher areas within the alluvium, is of

the *Dicanthium-Cenchrus-Elionurus* type. The *Elionurus* is especially good for grazing.

Undisturbed by man, these forest and grassland areas above the active flood zone were in all probability dense and flourishing. In case of biotic interference by man, the grasslands would succeed the forest areas, but these would in turn be reduced if used for cultivation or overgrazed (Puri 1960:30, 271ff.). Unless we assume climatic change in which increased rainfall would have permitted the growth of tropical savanna forests (the type most likely to have grown on the alluvial soil with less than 50 inches of rainfall), for which we have no trace, we must assume that the xerophytic forest species named above were dominant in Harappan times. However, riparian forests must always have existed in the Indus delta and in spots along water-filled meanders or near the Indus banks. (The best argument against the theory of significant climatic change in the region has been advanced by Raikes and Dyson 1961:265-281.) The importance of the latter to Harappan man, however, is not known.

The presence of acacias and euphorbia in the desert areas surrounding the lower Indus Valley proper seems to indicate biotic interference by man, in that these plants flourish when other vegetation has disappeared owing to overgrazing. This process still goes on in Sind, Rajasthan, and Baluchistan. It is impossible to ascertain whether it began in Harappan times, though there seems little doubt that it did in view of the archeological evidence that sheep, goat, and cattle were raised.

A tradition in Sind is to call the Larkana area the "Island." The area is indeed higher than the surrounding region and rises above the Indus flood generally (Fig. 6.9). The drain-off to the western Nara above Rajo-dero in former times seemingly confirms the fact that the land between the western Nara and the Indus River flood plain was slightly elevated from above Larkana south to the Aral Channel breakthrough and the Laki Hills. This elevated region consisted of the bars, cover flood plains, and part of the meander flood plains. It was presumably the most densely forested region and, according to the present evidence, it was also the locale for human settlement.

Within the forested area and the adjacent grasslands, the ecologies supported the elephant, the Indian rhinoceros, the zebu, the buffalo, the tiger, and other animals. The hog deer, the swamp deer, and probably the lion are also noted among the big game. Reptiles included the cobra, the crocodile, species of water snakes, the python, and a softshell turtle (*Chitra indica?*). Most of these animals either exist in Sind today or were known there within the time of written records (Aitken 1907:48-61; Pocock 1939 41; Boulenger 1890; Minton 1962).

Our present evidence strongly suggests an environmental setting in Harappan times similar to that of today, but in the modern situation it has been greatly changed by man's biotic interference.

The term "Mature" has been used to label that stage of development of the Harappan civilization best represented at Mohenjo-daro by the so-called "citadel" complex of buildings, which includes the Great Bath, the Granary, and other monumental structures situated high on a mound on a series of platforms. The extensive occupation marked by the best-constructed houses, drains, and streets in areas DK, VS, and HR also belong to this period. The bulk of the famous seals, the most distinctive stone and metal sculpture, and the characteristic black-on-red pottery are artifactual representatives of the period. As pointed out above, a Mature Harappan phase is found in sites as far west as Sutkagen-dor on the Makran coast, as far east as Lothal in Gujarat, as far north as Harappa on the Ravi, and Rupar at the foot of the Himalayas in the northern United Provinces of India. Apparently it was a short-lived period among the Harappan village sites in Sind, especially those around Lake Manchhar and southward to the drainage of the Malir River region. The evidence suggests that during the Mature Harappan period there was widespread movement out of Sind to the north, south, and east. That this movement was quite rapid seems to be confirmed by the almost exact identification of a large variety of artifacts ranging from Makran to the Himalayas. Again, it should be emphasized that generally in those areas where there was an already established pre-Harappan settlement, the Mature Harappan stage was superimposed rather abruptly. This situation has been noted at sites as widely separated as Amri in the south and Harappa on the north.

THE DECLINE

What is the reason for this rapid diffusion at the climax period of the civilization? The answer is not readily apparent, but there is a strong suggestion of a growing population confronted by failing resources. As Wheeler has pointed out, "they were wearing out their landscape" (Wheeler 1959:113). But what is meant by

this expression? What was the man-water-land relationship which lay at the foundation of the Harappan civilization and in the end caused its decline? A glimmer of light has been cast on the problem by Revelle (1964) and by other reports.

A reasonable estimate of the population of a habitation site can be obtained by comparing excavated house plans with modern village houses in the same situation. According to the West Pakistan census, there is an average of five to six individuals per household within the average village of Sind and Baluchistan (see also Braidwood and Reed 1957).

Where we have been able to compare house plans at excavated sites such as Kechi Beg, Damb Sadaat, Nal (Surkh damb), Chanhu-daro, and Kot Diji, we can ascertain the approximate number of houses probably present at a given period in the total visible site by reference to the fraction of the excavated portion to the total visible mound (Table 2). Thus if we assume that six individuals occupied a house, we are able to estimate the total population of the site. Obviously there are pitfalls in this method. We do not know, for example, what proportion of a village site was given over to non-inhabited dwellings such as temples and storehouses, nor is the total area of the site always visible, since more frequently than not the level of the surrounding plain has risen. Nonetheless the purposes for which these population estimates are obtained require relative and not absolute data. The error is more apt to be on the conservative side, i.e., an underestimate.

According to this method of estimating population, a ratio of 800 square feet per person can be worked out. The total area of Mohenjo-daro, exclusive of the monumental structures of the "citadel," is probably approximately 5,500,000 square feet (Table 2); thus, we can estimate the population as being 41,250 persons—probably a conservative figure, as pointed out above. If we use a figure for a typical rural area (Larkana District, Sind) from a modern census of Pakistan, we can make a population analysis of some significance (Table 3). Most notable is the fact that less than half of the estimated population is a part of the productive work force. Based on a study of the nature

TABLE 2 POPULATION ESTIMATES FOR SELECTED SITES IN THE INDO-IRANIAN BORDERLANDS, BASED ON STATISTICS OF MODERN SETTLEMENT IN WEST PAKISTAN

Site	*Period*	*Approximate Size in Feet*	*Square Feet*	*Total Number of Houses*	*Estimated Population*
Kechi-Beg (Q14)	H-2, DS-I	210 × 120	25,200	35[a]	210[a]
Damb Sadaat (Q8)	DS-I-III	400 × 400	160,000	181	1,086
Mohenjo-daro[b]	Harappan	3000 × 2750	5,500,000	10,428	41,250
Ghazi-shah	Amri-Harappan	525 × 450	236,250	295	1,770
Amri	Amri-Harappan	1800 × 450	810,000	1,012.5	6,075
Kot Diji	Amri (Kot Dijian) Harappan	600 × 400	240,000	300	1,800
Lohumjo-daro	Harappan-Jhukar	900 × 600	540,000	675	4,050
Pir Lal Chatto	Trihni	475 × 420	199,500	249	1,494
Pandi Wahi	Amri	450 × 350	147,500	184	1,104
Chauro	Amri	500 × 300	150,000	187.5	1,125
Chanhu-daro (Majumdar 1934)	Harappan	1000 × 700	700,000	875	4,950
Judeir-jo-daro	Harappan	1800 × 1500	2,700,000	3,375	20,240
Harappa, Mound E	Harappan	1200 × 1800	2,160,000	2,700	16,200
Harappa, granary mound	Harappan	1020 × 960	979,200	1,224	7,344
Harappa (Total, exclusive of "citadel")	Harappan	—	3,139,200	3,924	23,544
Harappa, "citadel"	Harappan	1800 × 840	1,512,000		
Malir 2	Harappan	540 × 420	226,800	283.5	1,701
Nal Bazar	Harappan	375 × 405	151,875	190	1,140
Amilano	Harappan	330 × 345	113,850	142	852

[a]The unit estimates are 800 square feet per person, or six people per house, in rural areas. In Q14, 180 square feet equals 1/4 house, and 720 square feet equals one house; in Q8, 882 square feet equals one house. The figure of 800 square feet is the "average." (See Government of Pakistan 1962.)

[b]Exclusive of the "citadel" and the area between it and VS, DK, and HR. As only approximately one-third of the total site has been excavated, two-thirds have been added to include the whole site as it is known from surface remains.

of artifacts and the character of the site, a productive work force for that city can be worked out by reference to activities (Table 3). One of the generally accepted criteria for defining civilization is the number of non-farming specialists supported by a surplus of foodstuff obtained by the total society. The amount of surplus dictates the number of non-farming specialists the civilization can support. Obviously, a falling-off of the means of subsistence, for whatever reason, reduces the surplus, and the consequent strain on the economy causes movement away to areas offering better subsistence possibilities. Significantly, it is the farming population that is the first major unit of the society to move away, because they possess the means to develop new lands and to support their own closely related, non-productive young and aged. The specialists in the characteristic rural "civilization" follow the farmers, rather than the reverse. Farming methods and related traits are usually stable elements within the culture and thus may account in part, at least, for the astonishing identity of Harappan sites remote from one another. Of the total wheat production in modern rural West Pakistan, 15 to 30 per cent is sold outside the villages; the rest is consumed locally. In a poor year, this percentage is drastically reduced. It is doubtful, on the present evidence, that the wheat production of the flood plain in Harappan times was better than this

TABLE 3. MODERN CENSUS FOR LARKANA DISTRICT, SIND

	Approximations	*Percentages*
Total population	605,000	
Total labor force	230,000	38% of total population
Cultivators	179,000	78% of total labor force
Herders, fishermen, and others	5,000	2% of total labor force
Non-agriculture	46,000	20% of total labor force

Mohenjo-daro, non-agricultural occupations
- Administrative
  - Priests
  - Scribes and seal cutters
  - Musicians and dancers
  - Engineers
- Productive
  - Potters
  - Weavers
  - Brickmakers
  - Masons
  - Carpenters
  - Metallurgists
  - Traders

figure. Thus if one uses the modern figure for Larkana District, 20 per cent of the non-farming population had available probably about one-fourth of the total crop, a fraction that a forced reduction of the total subsistence base through flooding or other disaster would drastically reduce. In the struggle for survival the farmer would be motivated to move elsewhere, followed finally by non-productive specialists. Thus we have apparently one factor that, this evidence suggests, helped to motivate the rapid move of the Harappans along and away from the Indus River Valley.

Food scientists have estimated that about 2300 calories is the minimum basic food requirement to sustain a working man, a quantity unfortunately not available to many on the subcontinent, even today (data on calorie needs as well as calorie content of foods drawn from Bowes and Church 1963; United States Department of Agriculture 1960; Revelle 1964). To assess the role of dietary habits and food requirements and the demands on the Harappan economy, we have used 2500 calories per individual per day as a reasonable low estimate of the food energy available. Since we have considerable knowledge of the food resources at Mohenjo-daro, we are able to work out a probable diet commonly available in Harappan times (Table 4). The significant figure is that for cereal grains, which provide more than three-quarters of the daily calorie intake. Again, the emphasis is on the ability of the farmer to produce, from one annual crop, sufficient cereal grain to fulfill this dietary requirement.

The advantage of setting up some reasonable picture of the daily requirement becomes clear when one attempts to ascertain the acreage necessary to support the estimated population. To provide an individual with 477.6 grams of cereal per day, it is necessary to produce 174,214 grams per year (365 multiplied by 477.6). According to Revelle and the writers of other reports, the wheat yield per acre is 8.7 maunds (Revelle 1964:90). There are 37,320 grams per maund, or 324,684 grams per acre (37,320 multiplied by 8.7 maunds). Thus one acre fed annually 1.86 individuals (324,684 divided by 174,214). This figure compares favorably with figures obtainable for West Pakistan today (Las Bela, 1.19; Larkana District, 2.27; sub-montane West Pakistan 1.0).

The wheat acreage estimated for Mohenjo-daro is 22,715 acres (41,250 divided by 1.86). This is of course exclusive of the acreage necessary for other crops, but, since their dietary contribution, though necessary, is not on the same scale as the wheat, on the present

TABLE 4. POSSIBLE INDIVIDUAL DAILY DIET IN ANCIENT TIMES

Foodstuffs	*Calories*	*Grams*
Cereal	1500-1600 (1617)[a]	477.6
Vegetables	50 (31)	14.8
Fruit	50 (32)	76.9
Oil, seeds, and fats	100 (89)	14.6
Sugar[b]	100 (137)	33.3
Meat and fish[b]	200 (24)[c]	160.0
Dairy products	125 (125)	156.2
Other[d]	375	?
Total	2500-2600	933.4

[a]Figures in parentheses record the diet of an average Pakistani today, which includes rice (Government of Pakistan 1960)

[b]Fish and honey do not require cultivated acreage. We can assume that fish was probably not an important part of the daily diet in farming villages, though it was consumed often during the year. Thus, these estimates are primarily based on beef and mutton consumption. However, sugarcane and sugar beets, available today, are not indicated before 1000 B.C.

[c]In both India and Pakistan the consumption of beef and mutton is minimal because of religious taboos, expense, and other factors. The eating of meat in ancient times is, however, confirmed by both traditional records and the bone remains in excavations.

[d]This category includes both domestic and wild vegetables, nuts, games, shellfish, and the like. So far their use has not been confirmed by archeological evidence, but some of these were, with some certainty, part of the diet. Eggs may also have been included in the daily diet. Chicken is in evidence at Kalibangan but not at Mohenjo-daro.

evidence their acreage requirement is omitted here. Suffice it to say, therefore, that the above figure is an underestimate. Increased acreage requires more productive energy and more available land.

The relationship of cattle to man provides the best insight into the problems of the economy of Mohenjo-daro. Striking among the remains of the period are the depictions on seals and in clay figurines of two kinds of cattle: humped species (*Bos primigenius*) and a short-horned species (zebu) probably identical with that found in western Asia. The water buffalo is present. Cattle remains are found in both Harappan and pre-Harappan sites in Sind and occur in the earliest deposits of settled life now known in Baluchistan (Kili Ghul Mohammad I). At the site of Sur Jangal in Loralai District of Baluchistan there is a suggestion that cattle replaced goats and sheep in the economy (Fairservis 1959:299-300). In South India, Allchin (1963) has found definite evidence of a widespread cattle-herding culture which may well date before 2000 B.C. The importance of cattle in Harappan times can hardly be underestimated on the present evidence.

Cattle fulfill two basic needs. One is as a source of food with, more than likely, some emphasis on dairy products. Their secondary use is as a source of energy, particularly for ploughing. An interesting aspect of the modern use of milk in West Pakistan is that 60 per cent of all milk goes to human consumption and 40 per cent to calves. These figures again emphasize the amount of production necessary to provide for both the sustenance of the food-producing resource and the excess to be used by man. In West Pakistan, of 9,000,000 cattle, 17 per cent are producers of milk, a third are calves or young without calves, and one-half are work animals. In Khairpur State a ratio of one bullock per eight cultivated acres has been worked out (Revelle 1964:204) as 0.13 bullock per acre. The ratio of cows to bullocks is 0.2.

Based on our estimates of cereal acreage needs and the daily dietary requirements, we are able to arrive at an estimate of the cattle population (Table 5). However, we must first assume that, in addition to cereal acreage (22,715), some effort was made to grow fodder. Both *bajra* (sorghum) and *jowar* (millet) are important fodder crops of the *rabi* season; both these crops are attested to have been grown in ancient times elsewhere (Helbaek 1960; Murdock 1959:68; Goodrich 1943:175). Whether one or both were grown in the Indus River Valley is unknown. Sorghum seems to appear much later in antiquity than does millet. Both of these cereal crops are reported considerably later than other cereals. Therefore, it is entirely possible that neither was grown in Harappan times. However, today, fodder acreage is 12 per cent of the total acreage cultivated or, at Mohenjo-daro, 3097.5 acres. Thus, the total acreage cultivated can be estimated to have been 25,812. This is exclusive of the possibility that cotton was grown and was the basic cloth fibre used. So far no evidence for the cultivation of flax has been found, but bast fiber was used in Baluchistan (Bird 1956). Possibly cotton or other fiber acreage is omitted because we have no data on which to base such an estimate for either modern or ancient acreage. In any case the addition of these data will increase the totals, which emphasizes the fact that the present estimates are probably underestimates.

Straw is the basis of dry fodder. The ratio of stalk to grain with *jowar* and *bajra* is 3.5/1, according to modern studies of cattle usage in Bihar, the nearest documented situation available (Fahimuddin 1963:66). (We should also note the absence of paddy in ancient times which today forms a substantial portion

TABLE 5. ESTIMATED VITAL STATISTICS FOR MOHENJO-DARO, INDUS FLOOD PLAN

GENERAL						
Acres, Sq. Ft.	No. of Houses	Acres Cultivated	Individuals Per Acre	Wheat Acreage	Fodder in Acres	Annual Fodder Requirements, Tons
5,500,000	3164	25,812	1.86	22,715	3097.5	41,535

CATTLE			
No. of Cattle	No. of Work Bullocks	No. of Milk Cows	No. of Cattle Young and Aged
8754.8	3226.5	2610	2918.3

FODDER, IN TONS			
Annual Fodder (Wheat/straw, 1/1)	Fodder Cultivation, 12% of Total Acreage	Cultivated Fodder	Grazing for Forage
8130.1	619 5	8749 6	32,785.8

POPULATION									
Total	Males	Females	Males, Ages 0-9	Females, Ages 0-9	Males, 60+	Females, 60+	Productive Population Males	Productive Population Females	Total
41,250	21,792	19,458	7527	7122	14.6	1070	12,849	11,266	24,115
	52.5%	47.5%	35%	36.6%	6.5%	5.5%			

DAILY DIETARY REQUIREMENTS PER INDIVIDUAL, IN CALORIES (TOP) AND GRAMS (BOTTOM)								
Diet	Cereal	Vegetables	Fruit	Oil and/or Seeds	Sugar	Fish and Meat	Dairy Products	Other
2500	1600	50	50	100	100	200	125	375
933.4+	477.6	14.8	76.9	14.6	33.3	160	156.2	—

of modern fodder production.) Wheat straw, on the other hand, is calculated on a basis of 1/1. We can calculate the available wheat, since we have an estimate of the annual wheat production [22, 715 = 197,620.5 maunds, or 16,260,215 pounds (rounded off) or 8130.1 tons]. Thus 8130.1 tons of wheat-straw dry fodder were possibly produced annually. *Jowar* production is 445 pounds per acre annually, and *bajra* is 345 pounds per acre annually. If we estimate 400 pounds of *rabi* dry fodder per acre as an average, we arrive at an annual yield of 619.5 tons of dry fodder (3097.5 acres by 400 pounds). The estimated total dry fodder yield is thus 8749.6 tons (8130.1 divided by 619.5).

According to our figures, the number of work bullocks necessary to cultivate 28,812.5 acres is 3226.5 (25,812.5 divided by 8), and the equivalent number of cows per bullock is 645.3. Thus 3871.8 cattle were necessary for the work energy required. The daily dairy-food requirement per individual was 156.2 grams, or 57,013 grams per year (we round off to 57,000 grams). The lactation period of a cow is 300 days. Based on an average of 9 pounds of milk per day per cow (4082.4 grams), we arrive at an approximate annual figure of 1,500,000 grams of milk per cow. If we use the figure of 60 per cent for human consumption (Revelle 1964), we arrive at 900,000 grams per cow as the annual amount available (600,000 grams to calves). If each individual needs 57,000 grams of milk annually, each cow provides milk for 15.8 people each year. Thus, 2610.7 cows are needed for a population of 41,250 people (41,250 divided by 15.8). In accordance with modern estimates, we must add one-third more to this cattle population in order to include old animals, calves, and cows without calves. The total cattle population can now be estimated, as follows:

Work animal	
Bullocks	3226.5
Cows/bullocks	645.3
Total	3871.8
Milk production	
Cows	2610.7
Less cows already in hand	645.3
Total	1965.4
Milkless cows, calves, and others (one-third of total above)	2918.3
Total cattle	8755.5

The daily fodder requirement for cattle on the basis of modern requirements for low-weight cattle is 600-700 pounds (Revelle 1964: Table 5.16; Fahimuddin 1963:68-77).

	Daily	*Annually*
Fodder Requirement (in pounds)		
Dry	10	3650
Green	15	5475
Other (concentrate)	1	365
Total	26	9490

Thus, the total annual fodder requirement is 41,535.4 tons (8755 multiplied by 9490 equals 83,070,950 pounds, or 41,535.4 tons). As described above, the total fodder yield, as we can now estimate it, was only 8749.6 tons, or 32,785.8 tons short of the basic requirement. If our figures in any way approach reality, the inhabitants of the mature period at Mohenjo-daro would have grown only about one-fourth of their fodder needs. It follows that the remaining three-quarters had to be obtained by foraging in the surrounding forests and grasslands. This formidable assault on the indigenous flora most certainly affected the ecology and had an adverse effect on the land and aided the spread of the active flood plain.

Like the cattle, man himself drew heavily on the local forests for fuel. Though it is difficult to estimate the ratio of fuel to quantity of brick, the enormous amount of fired brick that constitutes the visible site of Mohenjo-daro makes a marked impression on the visitor. Estimates of as many as 5,000,000 have been made for the number of fired bricks on the site. Personal observation of the use of *kandi* wood as a fuel for firing modern brick in Sind indicates that a mature tree provides enough fuel to fire about 1000 bricks in modern kilns. This rough ratio suggests a drain on the available forest resources, even if the estimated total of bricks at Mohenjo-daro were lowered by two-thirds. Raikes and Dyson (1961:276) suggested that 400 acres of gallery forest would have been sufficient for a rebuilding of Mohenjo-daro at intervals of about 140 years. This calculation of 100,000 bricks to 80 to 100 tamarisk trees agrees with my own observations. The use of manure as fuel would certainly lower the demands on the local forests, as Raikes and Dyson suggested (see also Dales 1962a:33).

This application of modern data to an assessment of the ancient situation produces a graphic estimate of what must have been difficult ecologically and economically. The levelness of the plain of northern Sind, with its native vegetation denuded and the surface of the ground exposed by man, beast, and wind, increased the active flood zone and therefore endangered the cities and villages. Conversely, the growth of population, human and animal, dependent almost solely on a *rabi* crop, created seasonal stresses which in the end caused the abandonment of most of the region.

At Mohenjo-daro, Jhukar, Chanhu-daro, and Amri there are evidences of a gradual deterioration in the last phases of Harappan occupation. On the other hand, at Kot Diji, Naru Waro-dharo, Ghazi Shah, Pandi Wahi, and other sites, only one or two phases of Mature Harappan occupation are found, suggesting that these sites were abandoned quite rapidly. Indeed, Agrawal (1964:950-951; see also Agrawal, Kusumgar, and Sarna 1964a:42), on the basis of radiocarbon dates, has indicated that the period covered by the Harappan civilization must be shortened considerably to fewer than the estimated 1000 years that are commonly given.

The evidence points to a precarious economic situation as a significant reason for the downfall of the third of the world's earliest civilizations. Even the great Punjab city of Harappa was abandoned after the Mature phase, as if the lessons of Sind had had no meaning. The administration of the Harappan cities was apparently ineffective in handling the problems. It may have been preferable to move away rather than to remain, which indicates that the rigid control by a theocratic or secular government, suggested by some authorities, was in fact largely non-existent. Raikes (1965c) has stressed the theory of a rise in sea level from tectonic causes, producing a pooling of the waters of the Indus River in Sind, and thus causing the abandonment of Harappan settlements. However, it seems that the vagaries of a mature river in its flood plain would be sufficient to cause the abandonment of traditional sites.

CONCLUSIONS

On the present evidence, the Harappan civilization cannot be said to have originated through direct or even stimulus contact with Mesopotamia, even though civilization is earlier in the latter region. The evidence strongly suggests that a concentration of developed village settlements in the highlands of the Indo-Iranian Borderlands provided interactions that made for a

constantly improving and intensified method of using local soil and water resources as well as advancing technology and creating social and political institutions necessary for civilized beginnings. The construction of dams and, with these, the fullest use of available land, the use of cattle for ploughing, the development of a mutually intelligible system of signs and symbols, the creation of local surpluses, which provided for the support of professional craftsmen, the institutionalizing of religious forms, and the development of domesticated animal breeds adapted to the ecology of the Borderlands (example: *Bos indicus*) are suggested as significant pre-Harappan advances in the highlands of the Borderlands. Essentially highland village cultures were the first to move into the Indus River Valley itself in pre-Harappan times. We do not know how long a period of experimentation and of minor settlement in the valley was involved. Absent from our evidence, for example, are the botanical data which would demonstrate the variety and number of changes necessary for the adaptation of highland or essentially Iranian varieties of cereal plants to the situation in the Indus River Valley. This had to be an essential of highland village settlement there.

The evidence indicates that settlement, once it occurred in the Indus Valley, was concentrated in northern Sind and on the slopes of the Khirtar Mountains at the western edge of the valley, whence it spread south and east. However, the largest concentration was on the southern borders of the great alluvial plain of Kachhi, where water was obtained from both the nearby drainage of Baluchistan or a branch of the Indus River emerging above Sukkur. Here occurred the characteristic style that we label "Harappan." The cultural florescence of the bearers of that style was rapid (probably no more than 200 years) and resulted in the creation of the great urban centers of Mohenjo-daro and, later, of Harappa.

It seems clear that the natural soil and water advantages of the Indus Valley made this florescence possible, i.e., the amplification of institutions already established. For example, a richer return per acre planted resulted in larger surpluses which in turn supported greater populations and permitted an elaboration of non-farming specializations. Thus the village priests became a priesthood, the metallurgist became one of many, as did the potter, weaver, seal-cutter, and the like. More private buildings and larger public buildings were mandatory in such a situation. The use of fired brick instead of mudbrick in building foundations may have been a response to the challenge of the flood potential of the Indus Valley. Village silos became urban granaries. Village ablutions and dances became temple rites. In effect, these elaborations had been motivated at the village level and given foundation for development in the endless potential of one of the richest alluvial tracts on earth.

The Indus River Valley is a transitional part of the Indo-Pakistan zoogeographic province. Highlands farming cultures adapted to that ecology lost their Iranian aspect and emerged Indian in form, following whatever local population with which they were in contact. The Harappan civilization presents an almost totally non-Iranian quality. Factors still to be defined were at work. We question the reasons for the heavily ornamented female figurines, the painted bulls, the seal writing and seal motifs, the great drains in and among building, the town planning, the repetitive themes in pottery, toy carts, settlement pattern, ornamentation, and many additional traits—all of which are virtually unknown, or exist in other forms and shapes, on the Iranian plateau. Clearly motivations within the life of the time made the change from Iranian village patterns to Indian "urban" styles even more dramatic.

The evidence again demonstrates a failure to come to grips fully with what must have become an increasingly acute situation—the destruction of the local ecological patterns and the consequent failure of food resources. Everything we know about the civilization indicates that the bulk of the population moved away to the north and south, still generally maintaining the characteristic "urban" village patterns. The uniformity of Harappan village sites wherever they are known indicates both rapid movement away from the traditional homeland and the maintenance of what can only be called the folk order in the fullest Redfieldian sense. Here is the firmest clue that we have to the character of the Harappan civilization. We cannot envision the civilization as representing a vast empire with twin administrative capitals, a fully developed riverine commerce, and a flourishing sea and overland international trade, as some authorities suggest. The conformity of Harappan sites and associated artifacts argues for the full syncretization of foreign elements. The centralization of administrative control obviously required to build large structures, and to cultivate acreage, harvest, and store is not beyond the powers of an elaborated village administration, though certainly adjustments were necessary, for example, in recording storage quantities and noting flood levels. But there is

no good evidence for priest-kings, slaves, courts of officials, and standing armies. When the Indus River Valley was overexploited and the return to man became less than his needs, no massive effort requiring the concentrated energies of thousands of persons seems to have been made, unlike that at Sumer, where great irrigation canals were constructed as a means of broadening the economic base. The Harappans apparently did not expand their cultivated acreage much beyond the active flood plain, even though the richest alluvial lands were available to them beyond that plain. Instead, the population appears to have drifted away northward to the Punjab, southward to the sea, and eastward to the Ganges and Gujarat, leaving only a remnant population to live out their lives atop the ruins of the old farming "cities." By doing so, they were conforming essentially to the same pattern that had been followed by their Iranian forebears in the past millennia. It was a civilization with cities but was not, at least politically, a state. Thus, it was neither Sumerian-like nor Egyptian-like but stands forth unique in its civilized character.

The ultimate and perhaps most telling evidence bearing on the character and history of the Harappan civilization is Indian civilization itself. The concept of Redfield and Singer (1954; see also Redfield 1953b) of primary and secondary civilization provides theoretical reasons for this conclusion. On these terms the evidence we have indicates that a "great tradition" marked by both urban and rural elements evolved out of hybrid "little communities," characteristic of the pre-Harappan Indus Valley. These were not isolated elements but were interrelated; in the Mature Harappan stage of development they had been transformed into an indigenous or "primary" civilization in which village and city alike shared a common culture. Such contemporary cultures as the Kulli in Baluchistan are probably variations on the "great tradition."

Redfield's (1953b:67) concept of a folk order moral in character is applicable here: ". . . the naive moral order of the folk followed by the addition of a public and state-managed moral order with speculative intellectual development, accompanied or followed by more or less syncretism of foreign elements with native elements—is probably characteristic of the rise of any indigenous civilization."

Our evidence supports this statement in part. The elements of the folk culture found with settled life in the Borderlands are more characteristic of the Harappan civilization than the foreign traits that were received and duly absorbed.

Marriott (1955) has discussed the applicability of this concept to a small community in North India and its relation to the great tradition of an indigenous or primary civilization that is Sanskritic Hindu. He concluded in part: "Viewed from the perspective of Kishan Garhi, the villages which are the little communities of India today may be conceived as relative structural nexuses, as subsystems within greater systems, and as foci of individual identification within a greater field. They cannot be conceived as things in themselves in their organization of marriage and kinship, residence patterns, modes of conflict, or caste organization. Nor are they ever likely to have been conceivable as isolates since Indian civilization began. The traditional social structure of the greater community of India similarly cannot be understood as apart from its continuing existence in relation to hundreds of thousands of little communities. Both little communities and greater communities are mutually necessary conditions of each other's existence in their present forms. One must consider both in order thoroughly to understand either."

We cannot help but feel that the evidence presently at hand places the Harappan civilization firmly in its Indian sphere. More than that, its institutions may in all probability lie at the very base of the Indian cultural continuum. The archeological evidence proves how much was shared by rural and urban communities as early as 2000 B.C. It also indicates how widely the Harappan cultural forms were dispersed. Marriott's conclusions bearing upon the complex but nonetheless formal and definite unities that are part of the Indian ethos are suggested by our evidence. On this basis perhaps we must turn more firmly to the Harappan civilization to view the origins of at least some major traits of Indian civilization. Probably the great gap between this ancient civilization and medieval India that we are prone to emphasize does not exist. If we direct our attention to these possibilities, we may be able to define far more of India's beginnings than has hitherto been possible. In any case, our efforts must be directed even further afield to understand the Harappan civilization on its Indian terms.

Present State of Research on the Indus Valley Civilization

M. RAFIQUE MUGHAL

A great deal of research has been done on various aspects of the Indus Civilization since its discovery fifty years ago, but much remains to be accomplished to enhance our understanding of this highly developed, fully urbanized and most extensive civilization of South Asia. During the last fifty years, most research in the Indus Valley proper, and the adjoining Indian territory, was oriented towards further elaboration of certain specific aspects of the Indus Civilization. With new discoveries, the interest of scholars has been further intensified and they are now offering fresh interpretations of both old and new data, and are asking many new questions about cultural and chronological reconstructions. In this process of healthy debate, we may agree or disagree with any or none of the views expressed, but it is all indicative of the world-wide interest which the Indus Civilization has aroused. This paper is intended to review past developments in research which have contributed to our understanding of this Civilization in relation to its origin, growth and decline, and also its geographical extent within and beyond the Indus river valley proper, during the third and second millennia B.C.

The initial impact of the discovery of identical remains of material culture at two widely separated and very large settlements, Harappa and Moenjodaro,[1] was considerable and immediately led to prolonged excavations at Moenjodaro between 1922 and 1931, first under the general supervision of Sir John Marshall (1931), and then by E.J.H. Mackay (1938b). It goes to the credit of a succession of early excavators of the old Archaeological Survey of British India, to uncover a considerable part of the city and to collect a wide range of material equipment. The emphasis has been, understandably, to expose the city plan and recover as many antiquities as possible to enable the excavators to present a fairly good picture of the civilization's material aspect. The same trend was witnessed at Harappa where, between 1921 and 1937, D.R. Sahni, M.S. Vats and K.N. Sastri uncovered the city plan with a citadel, recalling, in general lay-out, that of Moenjodaro (Vats 1940). However, some specific details of the buildings and, especially, the stratigraphic position of artifacts, necessary for understanding the growth of the cities, were left out by early excavators. In 1946, Sir Mortimer Wheeler added new dimensions to the defensive aspect of the Indus Civilization by excavating the huge mud-brick fortification at Harappa (Wheeler 1947a). The search for a similar feature led him to excavate at Moenjodaro in 1950, where he and his Pakistani colleagues also tried to reach the waterlogged lower levels near the citadel mound (Alcock 1952). But the problem of the high water-level frustrated all attempts to plumb the earliest horizons. During the excavations of 1964, Dr. George F. Dales carried out three test borings at the site which indicated signs of an ancient habitation 39 feet below the present surface on which the existing mound, 35 to 40 feet high, stands (Dales 1965b, 1965d, 1965c). Thus, we are made aware of the fact that the excavated remains at Moenjodaro represent about half the total history of its growth. Unfortunately, the gradually rising level of the ground water is progressively complicating our efforts to probe the early horizons of Moenjodaro.[2]

When large-scale excavations were in progress at Moenjodaro and Harappa, N.G. Majumdar located a number of settlements contemporary with Moen-

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jodaro, and even of an earlier date, during his explorations in 1927-31 along the west bank of the Indus in Sind (Majumdar 1934; Deva and McCown 1949). Later, in 1942, numerous settlements of the Indus or Harappan culture were recorded by Sir Aurel Stein along the dried bed of the Ghaggar-Hakra river in the central Indus Valley near Bahawalpur (Stein 1942; Field 1959:162-77 and 189-92). The number of Harappan sites in the Indian territory, as known in 1947, was few. The known sites, including the major ones, were mostly located in Pakistan, a situation which prompted Indian archaeologists to carry out an intensive search along the borders of Pakistan. As a result, an impressive number of settlements, contemporary with Harappa and Moenjodaro and of a later period, have been mapped in Rajasthan and Gujarat.[3] Three principal sites—namely, Kalibangan, Lothal and Rangpur—have also been subjected to systematic excavations for nearly a decade (Lal 1962; Rao 1957, 1962 and 1963a). Recently, in 1972, Mohammad Sharif of Pakistan's Department of Archaeology has recorded a large Harappan settlement, known as Gharo Bhiro, in south-eastern Sind, located on the route which was followed by the people of the Indus Civilization on their migration to Kutch and Saurashtra. Today, the northern-most limit of the Indus Civilization is not Rupar only, but extends up to the Gomal Valley, located at the eastern foot of the Sulaiman range where Dr. A.H. Dani has recorded a group of nine prehistoric sites, among which two sites, Gumla and Rahman Dheri, seem to be very important (Dani 1970/71).[4]

The settlement pattern of the Indus Civilization has not yet received serious attention. As the distribution of the Harappan sites reveals, the Indus Civilization was confined essentially to the vast plain of the *Greater Indus Valley*—the Indus and Ghaggar-Hakra river systems—and along the coast of the Arabian Sea. The Indus culture does not seem to have penetrated deeply into the Baluchistan hills. All the known sites, including those identified by the present author in 1972, are situated along the hilly borders, at the strategic passes and on the main overland routes, such as the site of Pathani Damb on the Mula river, Gudri in the Bolan Pass, Dabar Kot and Kaonri in the Loralai Valley, located on the ancient line of communication leading to southern Afghanistan, and Periano Ghundai in the Zhob Valley of northern Baluchistan, which joins the Gomal Pass. The evidence shows that the area covered by the Indus Civilization was larger than any of the known civilizations of the ancient world. Starting from the borders of Afghanistan, in northern Baluchistan (at Periano Ghundai), and the Iranian border on the Makran coast (at Sutkegan-Dor), it extended east and south-east and covered the entire Makran coast, the Greater Indus Valley and Gujarat. Beyond the vast plain of the Punjab, including that part which was formerly drained by the Ghaggar-Hakra river, remains of the Indus culture have also been found near Delhi in the Ganges-Yamuna doab (Fig. 7.1). This enormous area could not have been limited to only 144 sites (the number so far securely identified) and, indeed, claims have been made for the discovery of twenty-seven sites of Harappan affinity in east Punjab and the doab near Saharanpur (Fig. A).[5]

Lately, Indian and some Western, archaeologists have been insisting on a change of name, and favor the term "Harappa Culture" instead of the "Indus Civilization," since the former follows the site-name where the civilization was first discovered and recognized. However, despite the discovery of new sites in Indian territory, an overwhelming number of Harappan sites occurs west of the Ganges-Yamuna doab, in the vast plain drained by the Indus river and its present or former tributaries, which include the dried-up Ghaggar-Hakra river. The Rann of Kutch itself is formed by and lies in the Indus deltaic region. Thus, in addition to the archaeological evidence revealed by the distributional pattern of the Harappan settlements, there are geographical considerations to justify retention of the name, Indus Valley Civilization, which conveys the highly evolved expression of the Harappan culture. The spread of Harappan culture beyond the main Indus Valley should not astonish for, likewise, cultural traits of the Mesopotamian Civilization are found in Khuzistan in south-western Iran, in Saudi Arabia and along the Arabian coast of the Persian Gulf.

The distribution of the Harappan sites, at first sight, gives an impression of an Indus "Empire," encompassing Pakistan and the western part of modern India. Such an impression would be erroneous because we have yet to determine how many Harappan settlements were contemporary with each other at one specific time. Already, the Harappan remains excavated at several sites, some of which have been dated by radiocarbon tests, have indicated that most sites in the Kutch and Saurashtra area, and those which penetrated into the doab represent later movements of the people from the Indus Valley proper. It is, however, not yet certain what circumstances forced such

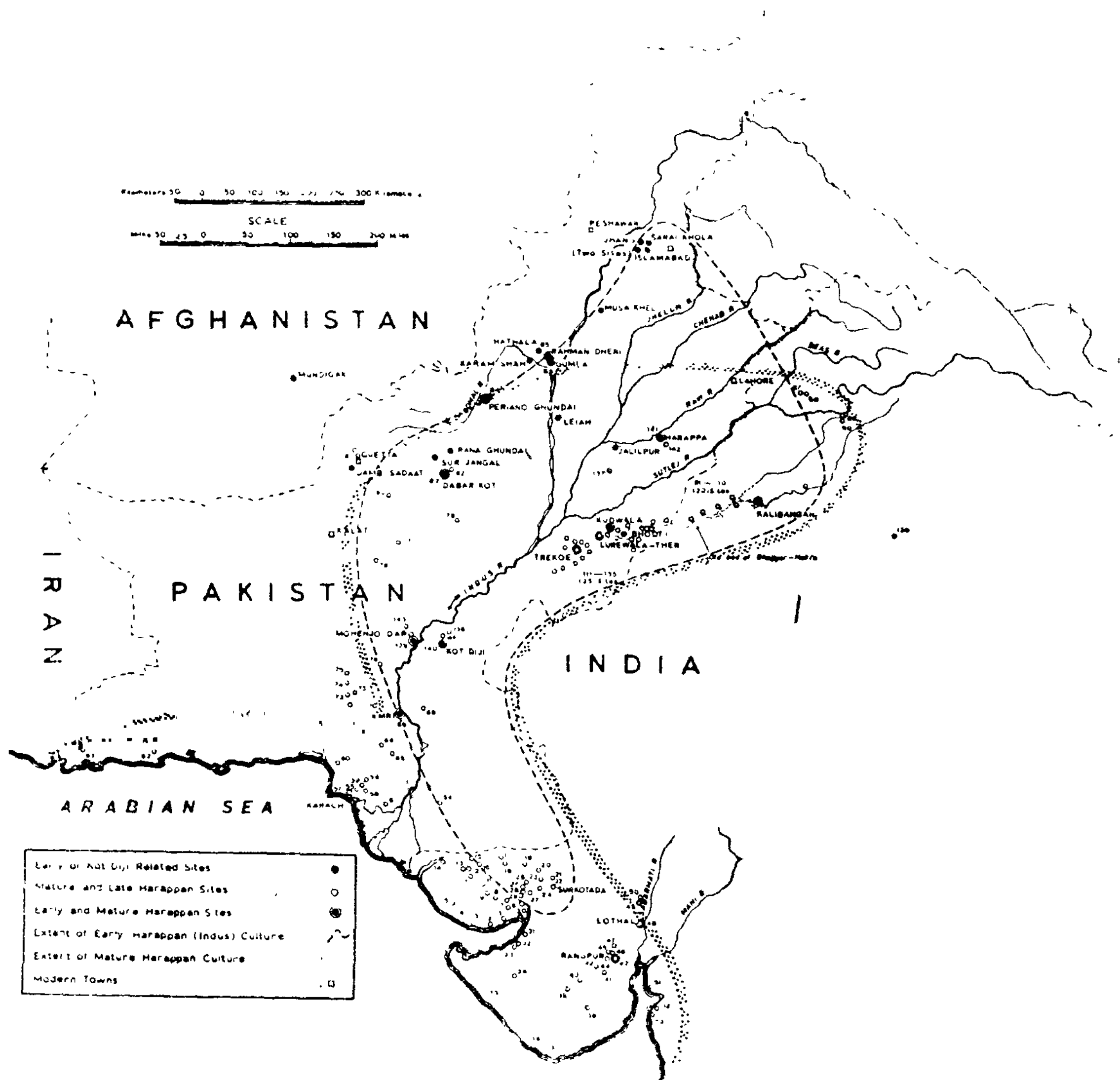


FIGURE 7.1. *The Indus Civilization during its mature and later periods covered the same area which was previously dominated by the early cultures (Amri, Kot Diji and Sothi) of the late fourth and early third millennia* B.C.

a migration and what fate befell the principal urban centers of Moenjodaro, Harappa and Kalibangan.

The picture of the Indus Civilization, as presented to us through many years of excavation, is that of a highly disciplined society, possessing sufficient economic wealth to mobilize labor and to support full-time craftsmen. It also possessed resources to engage in long-distance trade or exchange of products. The existence of interrelated but highly developed socio-political and religious institutions, as reflected through their well-planned cities, public buildings, large fortifications, granaries and standardization of material equipment through mass production, is evident. Further excavations at the same sites and new investigations at other sites are adding more detail to the picture. At most sites, the Harappan culture is encountered in its mature and developed form. This phenomenon has led to much discussion on the question of its origins.

The question of origins or the beginning of the Indus Civilization, it may be stated, has two aspects: (*a*) a clear delineation of those cultural materials which are chronologically earlier than the fully developed Harappan culture and a reconstruction of the early cultural phenomenon vis-a-vis the Indus Civilization; (*b*) reconstruction of processes which may have de-

veloped into urbanization. Due to lack of information relevant to this problem, it was popularly thought that, if not the people, at any rate, the idea or inspiration came from the west (i.e. Mesopotamia), where urbanization had taken place earlier than in the Indus Valley. This suggestion was based on certain tangible evidence of contacts between the Indus and Mesopotamian Civilizations. As regards the cultural phenomenon which existed in the Indus Valley prior to urbanization, investigations carried out at a number of early sites during the last fifteen years or so have provided considerable data which, ultimately, might alter our theoretical and conceptual framework regarding the emergence of urbanization in South Asia. It seems necessary to emphasize the relevant information on the fundamental question of the early developmental phase of the Harappan culture and its ultimate growth into full urbanization.

In 1946, a total of 191 potsherds of non-Harappan character were found at Harappa from the pre-defence levels and the building materials of the fortification wall. Among these, 35 came from two clearly stratified layers (26) and (26A) lying under the defence wall, and were thought to represent an occupation by a community having a variant or even alien culture (Wheeler 1947a: Figs. 8-9 and Pls. XL-XLII). This "alien" culture at Harappa received proper definition and elaboration by Dr. F.A. Khan's excavations at Kot Diji in 1955 and 1957. Kot Diji is a fortified settlement located on the left bank of the Indus river and opposite Moenjodaro (F.A. Khan 1965). There, below an occupation containing mature Harappan cultural materials, an accumulation of occupational debris, about 16 feet thick, revealed an abundance of pottery identical to that recovered from the pre-defence layers at Harappa. Although most of the Kot Dijian pottery forms reflect a basic difference in pottery tradition, some forms and decorative designs, such as fish-scale and intersecting circles, and other materials, namely terracotta toy-cart frames, wheels and triangular "cakes," which are otherwise considered to be the characteristic elements of mature Harappan culture, occurred in the mid-levels of Kot Dijian occupation. Two radiocarbon dates from the upper and lower (last but two) Kot Dijian levels in the citadel mound fall between 2605 ± 145 and 2090 ± 140 B.C., when calculated on a half-life of 5730 years, or 3155 and 2590 B.C. with appropriate MASCA factors added.[6] The first occupation in the unwalled outer area, corresponding to the mid-levels of the citadel, gave two C-14 dates: 2335 ± 155 and 2255 ± 140 B.C., or 2885 and 2805 B.C. with MASCA factors added. Chronologically, the priority of the Kot Dijian occupation over the succeeding Harappan culture is evident. The Indus Civilization is known to have been at a mature stage by 2371 B.C., contemporary with Sargon of Akkad in Mesopotamia. Seven carbon-14 dates are available from the upper levels of Moenjodaro which fall between 2083 ± 66 and 1760 ± 115 B.C., or 2583 and 2060 B.C. as adjusted.

The relevant information from the "early" levels of Moenjodaro itself has not received proper emphasis from scholars. It may be recalled that in 1932, E.J.H. Mackay excavated a small area, 18 × 14 feet, in the DK Area, Section G (House III, Room No. 76), down to 42 feet from an arbitrary "datum." The "early" levels, as defined by Mackay, start at the depth of 28 feet below the "datum." Some potsherds recovered by him between the depths of 31.8 and 42 feet were unlike most of the characteristic Harappan ceramics. Among a few illustrated examples, two are precisely identical in fabric and surface decoration with the so-called "wet" ware, commonly found in Damb Sadaat levels I and II in the Quetta valley and distributed extensively in northern and central Baluchistan (Mughal 1970:76-79).[7] Damb Sadaat I and II are assignable to 2510 ± 70 and 2200 ± 76 B.C. (or as corrected by MASCA to 3060 and 2700 B.C.). The most significant evidence is the occurrence of wet ware, of the type found in early levels of Moenjodaro, in association with the characteristic Kot Dijian ware at Damb Sadaat. It should be added that the famous piece of a carved steatite vessel having analogies with those from several sites in Iran and Mesopotamia was found along with "wet" ware at Moenjodaro. A similar change in ceramics was noted during the excavation of 1950. Pottery from the waterlogged levels, as reported by Leslie Alcock, was ". . . crude, vigorous, and unstandardized," and was thought to belong to a period ". . . far earlier than anything previously discovered on the site (of Moenjodaro)" (Alcock 1952). The available evidence pertaining to early Moenjodaro is limited but very intriguing, and raises the possibility of finding materials comparable to Kot Diji or related cultures, but this would involve deep digging, which can be undertaken only when present obstacles have been removed.

New field researches justify our affirmation that the Kot Dijian occupation was not an isolated phenomenon at Kot Diji only, but was widespread throughout the Greater Indus Valley and in the valleys of

northern and central Baluchistan (Mughal 1970: 98-100; Table 4; 156; 117-24). It extended even south-east up to the Great Rann of Kutch where, at Surkotada, J.P. Joshi is finding early materials lying below the Harappan remains (Joshi 1972b). Another astonishing feature, as demonstrated by the geographical extent of the Kot-Dijian-related early (Harappan) cultural assemblages is the fact that the very same area was later dominated by the mature Harappan culture (Fig. 7.1). To date, Kot Dijian ceramics or other related artifacts have been identified at nearly thirty sites and their number is likely to increase with further explorations. Some sites have already been excavated either extensively or briefly: Kalibangan by B.B. Lal and B.K. Thapar in 1961-1968; Mitathal and Siswal by Suraj Bhan in 1968 and 1970; Sarai Khola near Taxila by F.A. Khan and M.A. Halim in 1968-1972; Gumla in the Gomal Valley by A.H. Dani in 1971; Jalilpur in the central Indus Valley by the present author in 1971; and Surkotada, south of Rann of Kutch by J.P. Joshi in 1971-1972.[8] The evidence from Harappa has already been pointed out. To this list may be added Amri, located in south-western Sind on the right bank of the Indus, and long known to us by a distinctive group of bichrome wares called Amrian. The site was excavated for four years by J.M. Casal in 1959-1961, and four developmental phases of Amrian occupation have been identified lying below the Harappan settlement (Casal 1964a). From the middle levels of Amri, two radiocarbon dates fall between 2670 ± 113 and 2900 ± 113 B.C. (or 3320 and 3600 B.C. with MASCA factors added). The wheelmade Kot Dijian ware first appears in the earliest level IA at Amri among the dominant handmade Amrian ware. The inference would be that the radiocarbon date of 2605 ± 145 (or 3155 B.C.) from lower level (14) at Kot Diji might be too "late" and calls for reconsideration. The first occupation at Kot Diji might have begun towards the end of the fourth millennium B.C. At another contemporary site, Kalibangan, the earliest levels, occurring below the mature Harappan remains, produced pottery comparable with that from the pre-defence levels of Harappa and with Kot Dijian ware, along with a new white-and-black on red pottery called "Sothi." Eight C-14 dates from the Sothi levels or Kalibangan-I fall between 2370 ± 115 and 1825 ± 110 (or 2920 and 2125 as adjusted), a surprisingly low date. The Kot Dijian wares in Gumla levels II-III (underlying the mature Harappan occupation at that site) are assignable to 2248 ± 74 (or 2798 B.C.). It is important to note that some materials which are generally associated with the mature Harappan culture are found among these early Harappan cultural assemblages at all the sites, which were either reoccupied later by the mature Harappan culture (Kalibangan, Harappa, Gumla, Kot Diji and Amri) or were deserted (Jalilpur and Sarai Khola). Furthermore, despite regional diversity marked in pottery, there is a general pattern of uniformity in the ceramic types and other materials among these early Kot Diji-related settlements throughout the Greater Indus Valley. These similarities are not restricted to pottery alone; we observe that fortifications had already emerged at Kalibangan, Kot Diji and contemporary Amri-related sites of Kohtras Buthi and Pokhran in south western Sind, indicating that changes in the socio-economic structure had already begun. In domestic architecture, a stability of residence is suggested by prolonged and intensive occupation, and a gradual change was occurring from simple enclosures to multiroomed houses. A degree of class or occupational stratification is evident from the uniformity of ceramics distributed over a large area. The presence of the bull among found remains and terracotta figurines, toy-cart frames and wheels suggest the availability and use of means of transport for frequent interaction among various populations in the Indus. Long-distance trade or exchange is indicated by the presence at Sarai Khola, Jalilpur and Pandi Wahi, of lapis lazuli, originating from sources in Badakhshan in north Afghanistan. Thus, before the rise of large cities of the Indus Civilization, a widespread cultural phenomenon, constituting early, formative phase of the Harappan culture, had already set a permanent and uniform pattern of essential elements. It would seem that the processes leading to urbanization had already begun during the early third millennium B.C.; but it is not possible to reconstruct these fully in the present stage of our knowledge. Only two elements of the urbanized Harappan society of the mid-third millennium B.C. were lacking: (*i*) large cities and (*ii*) increased specialization to engage in full-time crafts like seal engraving, sculpture, metallurgy etc.

The location of both the early and mature Harappan sites, mostly along the former flood-plain of the rivers or permanent sources of water indicates favourable ecological conditions existing during the third millennium B.C., offering great possibilities of effective exploitation of land resources for sufficient means of subsistence. It is a question, therefore, whether a

possible migration of people from the Baluchistan hills, or beyond, generated the rise of urbanization in the Indus Valley. As regards the suggestion that the "idea" of civilization came from the west, it is argued by C.C. Lamberg-Karlovsky that processes towards urbanization in the Indus Valley and elsewhere could be generated by a series of complex interrelationships among populations maintained through trade or exchange (Lamberg-Karlovsky 1972a; 1972b).[9] It may be true for the early stages of the Indus Valley, where we notice that long-distance trade in lapis lazuli had already been established and that changes in social organizational structure were underway. However, more evidence is needed through archaeological and environmental studies to enable us to reconstruct evolutionary stages towards full urbanization. On present evidence, the mature Harappan culture at early Indus or Harappan sites arrives with a fully developed material culture. Does this sudden change mark a major shift in the socio-political structure of the Indus people? If so, what were the circumstances in which such a change occurred? At present, we have no evidence to answer these pertinent questions.

Where archeologists cannot provide satisfactory answers to certain specific questions, natural and physical scientists are helping us with their experience and specialized knowledge. Working independently or with archeologists, their contributions have been particularly significant in understanding the ancient climate and physical environment of the Indus Valley in early millennia, to determine possible causative factors leading to the decline of the civilization and in fixing the time range of the Harappan culture. As to the physical environment, it was generally thought that the Indus Valley must have enjoyed abundant rainfall in ancient times, necessitating a universal use of durable baked bricks in buildings and the provision of an elaborate drainage system. It was believed that only "wetter" conditions could produce vast reserves of forest for firing millions of bricks. The representation of animals such as the tiger, elephant and rhinoceros on seals would also suggest the existence of dense jungles in those times. Such inferences led to the presumption that climatic changes in the Indus Valley might have produced a drastic imbalance between human populations and their subsistence base, which ultimately hastened the disappearance of the Indus Civilization. Contrary to this idea, R.L. Raikes and R.H. Dyson now maintain that there has been no appreciable change in the climate of South Asia and also of the Near East in the past nine thousand years (Raikes and Dyson 1961). They argue that the amount of silt deposited annually along the flood-plain of the Indus would be sufficient to produce dense jungle growth and make it a suitable habitat for the animals depicted on the seals. Moreover, the small size of the drains in the streets of Moenjodaro and Harappa, with their limited capacity, would itself indicate their function of carrying water for domestic use only. Their arguments are generally accepted, though it is not certain whether the absence of a modern irrigation system would re-create suitable ecological conditions for supporting a civilization or whether the whole valley would be a desert waste assuming an annual rainfall, as at present, of not more than 10 inches. The evidence of an ancient irrigation system consisting of canals or other means is lacking.[10]

In recent years, the decline and end of the Indus Civilization has become a subject for considerable debate and sharp controversy. New avenues of enquiry are being opened, but we have still to understand how and why this highly developed and complex civilization disappeared. The popular theories of adverse climatic changes and invasions from outside are being disputed on the basis of geographical and geomorphological factors combined with archaeological evidence. At the same time, socio-economic models are being presented as the operative cause for the break-up of this civilization. It would appear that this heated controversy has much to do with the question of emphasis on this or that causative factor, or on a combination of many causes. Among various opinions, the Aryan invasion around the middle of the second millennium B.C. is considered to have hastened the process of disintegration of an already weakened civilization (Wheeler 1968:129-34). Alternately, it is asserted that we have to imagine that another civilization with fortifications arose in the Indus Valley which was encountered by the Rigvedic war-god, Indra, who is depicted as the destroyer of many forts. It is also thought that the people were already over-exploiting the land resources, and when the situation became critical with a possible increase in population numbers, they were forced to migrate to other areas (Fairservis 1971:296-311). Recently, Dr. G.F. Dales and R.L. Raikes have joined hands to tackle this problem (Raikes 1964; Sahni 1956; Dales 1966a; Dales and Raikes 1968). They postulate that the Lower Indus Valley was flooded due to tectonic uplifts somewhere between Amri and Sehwan. This

natural phenomenon necessitated the building of earthen embankments around the city of Moenjodaro and the frequent rebuilding of dwellings in view of advancing floods. This process continued until "a phase of rejuvenation of the Indus channel" developed. The civilization was weakened due to relentless pressure exerted on the population and their economic resources. There have been sharp criticism of this suggestion from G. Possehl and H.T. Lambrick based on the nature of the evidence used to support this theory (Possehl 1967; Lambrick 1967). Incidentally, Raikes' investigation in the east-central Punjab near Kalibangan has also indicated that, in ancient times, there have been frequent changes in the river course due to tectonic disturbances (Raikes 1968a). That the Indus Valley does fall within an active seismic zone was noted by Thomas Oldham a long time ago.[11] At present, however, whatever may be the merits or pitfalls of new theoretical frameworks, the happy collaboration of physical scientists with archaeologists in the solution of many a complex problem of the Indus Civilization is potentially significant.

On the question of the Indus contacts with contemporary civilizations, the basis of evidence previously rested on some seals of the "Indus" type, several scattered pieces of metal types, etched carnelian and other segmented beads, some pottery forms and their painted style, stone vessels and other minor objects like dice, figurines etc., mostly found in Mesopotamian contexts (F.A. Khan 1964; Dales 1968c; During-Caspers 1971, 1972). In view of extensive excavations already done in Mesopotamia and on Indus sites, the available number of dateable materials was extremely limited. This was all the more surprising in the face of frequent references to the sea-borne trade of southern Mesopotamia with distant lands in hundreds of economic and religious texts, sometime between the Sargon of Akkad and Ur III to the Isin-Larsa periods (c. 2370-1900 B.C.). However, despite difficulties of limited evidence, the subject of Indus-Mesopotamian relations or contacts continued to occupy the attention of many scholars. This aspect is, in fact, of growing concern among South Asian and Near Eastern archaeologists even today. During the last fifteen years, new evidence has been added to the existing list of objects used for exchange. Recent researches are also reflecting a trend towards explaining or understanding the mechanism involved or implied in Indus-Mesopotamian relations (Mallowan 1965; Lamberg-Karlovsky 1972a; Shaffer 1972). As the evidence goes at present, scholars seem to be in general agreement on the identification of at least two of the three principal territories—Dilmun, Magan and Meluhha—mentioned in the cuneiform documents, and which are known to have participated in trade with Mesopotamia during the third and early second millennia B.C. Meluhha is most likely to be identified with the territory encompassed by the Indus Civilization, including the coastal areas of Pakistan and Western India, on which a number of Harappan settlements are located. The island of Bahrain, located close to southern Mesopotamia and near the Arabian side of the Persian Gulf, fits well with the description of Dilmun and is also consistent with the archaeological evidence found recently. The exact location of Magan is not clear but it is generally thought to have included the south-eastern part of Iran excluding perhaps the then hostile territories of Elam in southern Iran (Sollberger 1970).

In 1932, C.J. Gadd pointed out stylistic similarities of a group of seals from Ur, and elsewhere in Mesopotamia, with those of the Indus Civilization (Gadd 1932). Now, more seals of the same category, which are neither wholly Mesopotamian nor Indus in character, have been discovered at Bahrain, Failaka and elsewhere on the Arabian side of the Gulf. Contrary to the familiar shape of the Indus seals, which are mostly square, the "Persian Gulf Seals" are circular, like those of the post-Indus (Jhukar) occupation levels at Chanhu-daro, and one reported from the surface of Lothal, a Harappan port-city in Saurashtra. It may be recalled that both Lothal and Chanhu-daro have also produced convincing evidence of a local bead-making industry (Mackay 1943:186-210 and S.R. Rao 1962:87).

Another denominator of Indus contacts with neighboring lands is a class of steatite vessels carved on the outer surface with a variety of hut, matt, animal and other designs. Their widespread distribution in the ancient Near East and their parallel with a fragment from an "early" level of Moenjodaro were summarized by F.A. Durrani some years ago (Durrani 1964a). In recent years, however, new fieldwork, done in the adjoining regions, has produced a wide range of carved vessels of stone, and thus suggests a wider geographical range of Indus contacts than was known previously. Further evidence comes from Shahdad in Dasht-i-Lut and Tepe Yahya in Iran, Tarut and Umm an-Nar respectively, off the coast of Saudi Arabia and Abu Dhabi, while similar designs, copied on grey pottery,

are reported from Shahr-i Sokhta in Iranian Siestan, Bampur and related sites in southeastern Iran, Hili and Umm an-Nar (Burkholder 1971; Porada 1971; During-Caspers 1970). The carved steatite vases are now securely dated at the newly excavated sites and are mostly assigned to the Mesopotamian Early Dynastic I-III period and even earlier, consistent with the evidence of Indus-Mesopotamian contacts existing during the third millennium B.C. Likewise, the evidence of distinctive "Persian Gulf Seals" would corroborate the documentary evidence indicating a flourishing sea-borne trade with Mesopotamia during the Ur III and Isin-Larsa period, ending around 1900 B.C.

Along with new evidence, research is now also oriented towards explaining or understanding the mechanism of East-West trade both by sea and overland routes. From the most recent evidence, it would appear that the early third millennium B.C. was a period of great cultural interaction among the populations of the Greater Indus Valley, Baluchistan, Iran and southern Mesopotamia. Sometime in the early second millennium B.C., contacts through overland routes were mostly disrupted and diverted to the sea-route via Dilmun. It is also being argued that the evidence of direct overland trade or contacts between the Indus Civilization and Mesopotamia is slight. Whatever tangible evidence is available does reflect an indirect exchange of certain specific items. On the other hand, in a recent reanalysis of the archaeological evidence pertaining mostly to the third millennium B.C., there appear to be strong links among the cultures of the Indus Valley, northern Baluchistan and regions beyond including southern Afghanistan and Turkmenia (Mughal 1970:311-23; Dales 1973; Sarianidi 1971a, 1971b; Masson and Sarianidi 1972:94-6). The intermediate region of central and southern Baluchistan is most likely to shed more light on the nature, duration and mechanism of Indus-Mesopotamian relations and contacts with the cultures of Iran. In this context, the decipherment of the enigmatic Indus script engraved on thousands of stamp seals could be substantially helpful. The attempts made so far by many scholars have not succeeded in breaking the code (Dani 1963; Blanke 1971).[12]

Regarding the time-range covered by the Indus Civilization, especially the mature period as known at Moenjodaro and Harappa, it remained fashionable for a long time to impose a Mesopotamian chronology on the Indus Valley based on stylistic parallels of material objects found at both places or elsewhere in the Iranian plateau. In recent years, most of the initial problems of doubtful stratified contexts of comparable materials have now been overcome by fresh evidence from new excavations, which are usually carried out carefully under closely observed conditions. But the relative chronology worked out by comparisons with distant lands cannot be regarded as a precise and useful frame of reference. In the absence of dated and documentary evidence from the Indus Valley and on account of the fact that the Indus script is still undeciphered, internal chronologies of the cultures in the Indus Valley, and of other South Asian sites, have not yet been properly reconstructed for understanding the developmental sequence of the material culture.

With the introduction of the C-14 method of dating charcoal, bones and shell since the 1950s, radiocarbon dates are being eagerly sought and used—sometimes without caution and subjectively—for determining the absolute chronology of a given site. Initially, C-14 dates did not fit into the historical dates derived from astronomical information, especially in Mesopotamia and Egypt. It was noted that C-14 dates of the third and second millennia B.C. time-ranges were consistently later than the calendrical dates. The radiocarbon method itself has since been further developed and refined. The most recent correction proposed by the scientists for the already available radiocarbon dates seem to correspond with the archaeological dates, as the recent reappraisal of the C-14 dates from South Asia by G.F. Dales clearly demonstrates. It must, however, be admitted that the number of dates available from the Indus Valley are few. Kot Diji was the first South Asian site to be dated by the radiocarbon method. Seven dates from Moenjodaro come from the upper levels only. Kalibangan is an exceptional site, from where thirty carbon-14 dates are available. A sufficient number of dates should be available from other Indus Civilization sites to enable us to determine the life span of each city and for the solution of other chronological problems.

These are, in brief, the main features of research done on various aspects of the Indus Civilization during the last fifty years. Our knowledge of this civilization has substantially increased; but, as pointed out at the beginning, we have yet to learn a great deal more about it. We still are unable to read the Indus script, and we do not have sufficient evidence to reconstruct those circumstances which produced urbanization in the Indus Valley. The total number of Indus

settlements is also not known. In fact, many generations of archaeologists, working in close collaboration with anthropologists and specialists in many other disciplines, are needed to unravel those aspects of this highly evolved civilization which have eluded us for half a century.

In conclusion, it should be added that Moenjodaro is the largest of all the known cities of the Indus Civilization. It is located in that part of the Indus Valley which may have been the core area of this civilization. Therefore, Moenjodaro possesses great promise of yielding clues to the early or formative phases of the Indus Civilization, but much depends upon whether the existing ground water-table can be lowered sufficiently to permit us to reach the earliest settlement, now unhappily submerged.

NOTES

[1]The spelling "Moenjodaro" is that currently followed in Pakistan.

[2]Various schemes for lowering the water table have been prepared and many UNESCO experts have studied the problem. See UNESCO (1964); Khan (1970b) and Department of Archaeology (1972).

[3]Many of these have been reported in *Indian Archaeology: A Review* in issues from 1953 to the present.

[4]The other Harappan sites include Hisan Dheri, Bud-ki-deherai and Mahra Sharif in the Gomal Valley, and Musa Khel thirteen miles north of Mian Wali and Laiya north of Dera Ghazi Khan. The latter two sites are located east of the Indus.

[5]About one hundred sites are listed in Wheeler (1968 :138-40). More Harappan sites, mostly unpublished, in the Bahawalpur-Bikaner area are listed in Mughal (1970:158-60). In addition to the sites in the Gomal Valley (Dani 1970/71) nearly one hundred sites are reported by Suraj Bhan in the east Punjab and northern Rajasthan (Suraj Bhan 1973). For the listing of sites in Gujarat see Rao (1963a) and in the Karachi area see A.R. Khan (1968). Pande and Ramachandran (1971 :37-42) include a listing of Harappan sites in all regions; however it is incomplete for those in Pakistan.

[6]All C-14 dates in this paper are calculated on a new half-life of 5730 ± 40 years instead of the 5568 ± 30 years as published in *Radiocarbon*. Recently, new correction factors were proposed by the Museum Applied Science Center for Archaeology (MASCA) of the University Museum, University of Pennsylvania: see Ralph and Michael (1971 :1-48) and Ralph, Michael and Han (1973). Editor's note: Ralph, Michael and Han has been reprinted in this book.

[7]For specific parallels compare Mackay 1938b: Vol. II, Plate LXVII, nos. 1-2 with Fairservis 1956a :268-70. These wares were also found by the author during exploration in Baluchistan during 1972.

[8]For Kalibangan see *Indian Archaeology: A Review 1961-62*, pp. 39-44; *Indian Archaeology. A Review 1962-63*, pp. 20-31; *Indian Archaeology: A Review 1963-64*, pp. 30-9 and *Indian Archaeology: A Review 1967-68*, pp. 42-5. For Sarai Khola see Halim (1972a and 1972b). For Gumla see Dani (1970/71). For Mitathal see Suraj Bhan (1969). For Siswal see Suraj Bhan (1971 72). The report on Jalilpur is under preparation by M R. Mughal

[9]Maurizio Tosi (1973a :444-6) however suggests Iranian and Turkmenian influences on the origins of the Indus Civilization.

[10]The palynological studies carried out in the east-central Indus Valley, however, indicate that the Indus Valley did enjoy greater rainfall during the third millennium B.C. than today and that a significant shift towards dry conditions occurred about 1800 B.C., coinciding with the decline of the Indus Civilization (Singh 1971).

[11]An important reference which supports the theory of tectonic upheavals not noted by Raikes is: *A Catalogue of Indian Earthquakes from the Earliest Times to the End of A.D. 1869*, published in Calcutta in 1883.

[12]Soviet and Scandinavian teams have been especially engaged in breaking the Indus Code with varying degrees of success. The initial attempts have been given in the *Scandinavian Institute of Asian Studies*, Special Publications, Nos. 1-2 in 1969 and No. 3 in 1970. These are superseded by *Materials for the Study of the Indus Script*. Helsinki, 1973 with a further bibliography.

Part III Papers of Historical Interest

Editor's Introduction

The first archaeological report on a settlement of the Indus Civilization was written by Sir Alexander Cunningham (1875) after his third visit to Harappa. In it he refers to earlier visits by Charles Masson in 1826 and Alexander Burnes in 1831. None of these men recognized the true antiquity of the city, assigning it variously to the Greek or Buddhist periods. But Cunningham published sufficient illustrative material for there to be no doubt that he was the first to excavate the remains of this civilization, since the square steatite seal (Fig. 8.1) on his plate of finds is clearly of Indus type.

The three papers which follow Cunningham's report on Harappa mark the beginning of modern scholarship on the Indus Civilization. Sir John Marshall's introduction is basically descriptive and outlines his view of the historical significance of Mohenjo daro, and South Asia's first civilization. The somewhat curious letter from A.H. Sayce, which appeared one week after Marshall's introduction, is the first document to draw explicit comparisons between the Indus Civilization and Mesopotamia. The Gadd and Smith article published on October 4, 1924, two weeks after the introduction, expands and explores the points initially noted by Marshall and Sayce.

Marshall's paper has now been eclipsed by five volumes of comprehensive reports covering the excavation at Mohenjo daro. Much of what Gadd and Smith have noted is also severely dated. The materials reprinted in Parts IV and V of this book deal with contacts between the Indus and Mesopotamia and adequately summarize the current state of our knowledge in this matter. But it is these four papers which mark the beginning of our knowledge of the Indus Civilization.

The three articles from the *Illustrated London News* carry with them at least one additional insight into the archaeology of the 1920s. It seems clear that the Gadd and Smith paper was written as a sequel to Marshall's. Why then the unnecessary interjection by Sayce? I suggest that this letter is best seen as a statement about the order of men in the British archaeological world of the time and may therefore be of interest to those concerned with the history of the discipline.

Harappa

SIR ALEXANDER CUNNINGHAM

The ruins of Harapâ[1] are the most extensive of all the old sites along the banks of the Râvi. On the north, the west, and the south, there is a continuous line of mounds about 3,500 feet in length; but on the east side, which is only 2,000 feet in length, there is a complete gap of 800 feet, for which I am unable to account. The whole circuit of the ruins is therefore about 12,500 feet, or nearly 2½ miles. The highest mound is that to the north-west, which is 60 feet above the fields. On the south-west and south the mounds range from 40 to 50 feet in height, and on the north side towards the old bed of the Râvi, from 25 to 30 feet. Burnes speaks of "*a ruined citadel* on the river-side of the town" (Burnes 1834, Vol. III:137), by which I suppose he means the *western* side, which is the most commanding portion of the ruins. Masson calls it "a ruinous brick castle," and distinctly states that it was situated to the west, on what he erroneously supposed to be a natural rocky height (Masson 1842, Vol. I:452), but which is only an accumulation of masses of brick walls and broken bricks. He describes "the walls and towers of the castle" as being "remarkably high, though, from having been long deserted, they exhibit in some parts the ravages of time and decay." I believe that Masson's visit was made in 1826; and just five years later, when Burnes was on his way to Lahor, the brick castle was already "ruinous." In 1853 I was informed that Mer Singh, a Sikh, had built a fort at Harapâ, some 70 years ago, or about A.D. 1780. Perhaps this was the castle seen by Masson and Burnes.

In 1853, and again in 1856, I traced the remains of flights of steps on both the eastern and western faces of the high mound to the north-west, as well as the basement of a large square building. Here the people say was the site of a great Hindu temple in the time of Raja Har Pâl or Hara Pâla. But there are no traces of any castle at the present day.

On the same mound, but lower down towards the east, there is a tomb of a *Naogaja* named Nûr Shah, which is 46 feet long and 3½ feet broad.[2] Besides the tomb there were formerly three large stones, which the people believed to be the thumb rings of the gigantic Naogaja.[3] These three stones are now on the plain below. The largest one is a black stone, 2 feet 9 inches in diameter, which is said to have been the *nag*, or gem of the ring. The other two are each 2 feet in diameter and 1 foot 1½ inch high, with a hole through the middle, 10½ inches in diameter. These are called *nâl* and *manka*, and are said to be the thumb rings of the giant. They are made of yellow ochreous limestone, and are very peculiar in shape, which is difficult to describe. They may be called undulated rings of stone; but the best idea of their shape will be derived from the accompanying sketch.[4]

About 70 feet still lower down the eastern slope of the mound there is a small ruined Idgâh, which is said to have been built in the reign of Akbar.

On the south face of the southern mound, there are the traces of a large square building with rooms on four sides surrounding a courtyard, as in a Buddhist monastery. The walls were very massive; but the whole has now been removed to form ballast for the Railway. Perhaps the best idea of the extent of the ruined brick mounds of Harapâ may be formed from the fact that they have more than sufficed to furnish brick ballast for about 100 miles of the Lahor and Multân Railway.

The people refer the ruin of Harapâ to the wicked-

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ness of a Raja named Har Pâl, or Hara Pâla, who was in the habit of claiming the sovereign's rights at every bridal. At last, in the exercise of this royal privilege, he committed incest with a near relative. Some say his own sister, others his wife's sister, or his wife's sister's daughter. The girl prayed to heaven for vengeance, and then the city of Harapâ was instantly destroyed. Some say it was by fire, and some by an earthquake; others say that an invader suddenly appeared, and that the city was taken by storm, and the Raja killed. The period of its destruction is vaguely said to be 1,200 or 1,300 years ago. If the date is correct, the city of Harapâ must have been destroyed by Muhammad-bin-Kasim in A.D. 713, just 1,260 years ago. I am inclined to put some faith in this belief of the people, as they tell the same story of all the ruined cities in the plains of the Panjâb, as if they had all suffered at the same time from some sudden and common catastrophe, such as the overwhelming invasion of the Arabs under Muhammad-bin-Kasim. The story of the incest also belongs to the same period, as Raja Dahir of Alor is said to have married his own sister.

In another place (Cunningham 1871:203), I have advocated the claims of Shorkot to be identified with the city of *Po-fa-to*, or *Po-fa-to-lo*, where the Chinese pilgrim halted for two months to study the principles of the sect of *Sammitiyas*. But I have now visited Shorkot, and I have satisfied myself that it never could have reached the size of 20 *li*, or upwards of three miles in circuit, which the pilgrim assigns to *Po-fa-to*, or *Po-fa-to-lo*. But the size agrees almost exactly with that of Harapâ,[5] and as the position otherwise corresponds, I believe that Harapâ must be the very city visited by the Chinese pilgrim. He describes the population as very dense. There were four stupas and twelve monasteries counting about 1,000 monks, besides twenty Brahmanical temples. Near the town there was a large ruined monastery which had been destroyed by lightning.

I am unable to offer any explanation of the name of *Po-fa-to*, or *Po-fa-to-lo*, which M. Julien has altered to *Po-lo-fa-to*, for the purpose of making it agree with a known Sanskrit word, *parvata*, or "mountain." But as Harapâ is in the open plain, this reading does not commend itself for acceptance. The name of *Bavanni* seems to offer the best equivalent of the first two syllables *Po-fa*, but *Bavanni* could never have been even one-half the size of the *Po-fa-to-lo* of Hwen-Thsang.

I made several excavations at Harapâ, but the whole surface had been so completely cleared out by the Railway contractors that I found very little worth

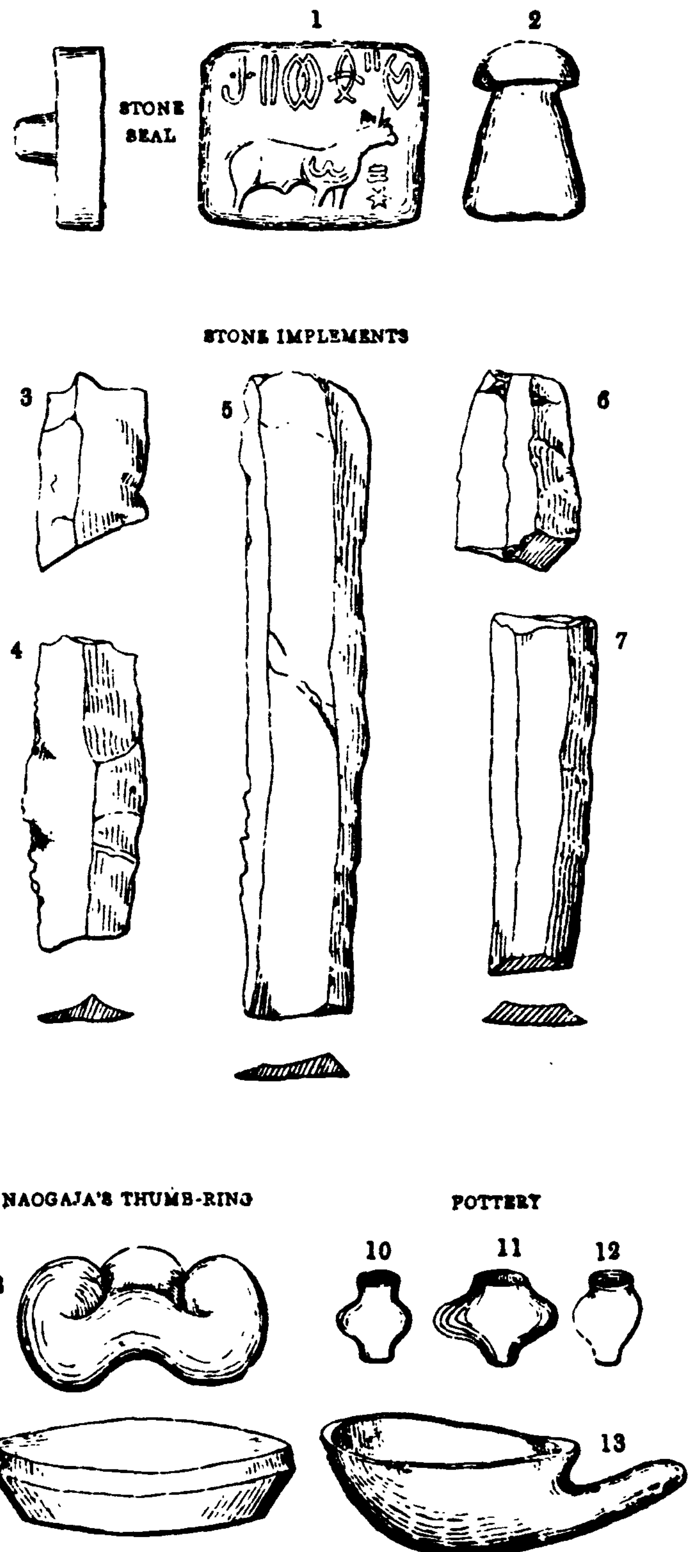


FIGURE 8.1. *Material recovered by Cunningham at Harappa in 1871-72.*

preserving. My chief discovery consisted of a number of stone implements for scraping wood or leather. Several specimens of these are given in the accompanying plate (Fig. 8.1, nos. 3, 4, 5). Most of them have one edge more obtuse than the other, with a flat surface between; but a few have the two edges alike, with a single ridge between them. They are nearly all made of a dull cream coloured stone, but a few of these are nearly black in colour.

My excavations also brought to light numerous specimens of ancient pottery, of which I have given some sketches in Fig. 8.1, no. 13 appears to be a clay spoon or ladle.

The most curious object discovered at Harapâ is a seal, belonging to Major Clark, which was found along with two small objects like chess pawns, made of a dark brown jasper. All these are shown in the accompanying plate (Fig. 8.1, nos. 1, 2). The seal is a smooth black stone without polish. On it is engraved very deeply a bull, without hump, looking to the right, with two stars under the neck. Above the bull there is an inscription in six characters, which are quite unknown to me. They are certainly not Indian letters; and as the bull which accompanies them is without a hump, I conclude that the seal is foreign to India.

Harapâ has yielded thousands of coins of the Indo-Scythians and their successors; but I am not aware of the discovery of a single Greek coin.

NOTES

[1]The archaic spelling of the site name has been retained for this paper.

[2]Burnes, by some oversight, makes the tomb only 18 feet in length (1834, Vol. III:137).

[3]See Mason (1842, Vol. 1:452) "I examined the remains *on the height*, and found two circular, perforated stones " See also Burnes (1834, Vol. III:137): "A large stone of a circular form and a huge black slab of a novel shape *which lie near* the grave. . ."

[4]See Figure 8.1, nos. 6 and 7

[5]Burnes (1834, Vol. III:137) makes the circumference of Harapa "about three miles."

First Light on a Long-Forgotten Civilization

SIR JOHN MARSHALL

Not often has it been given to archaeologists, as it was given to Schliemann at Tiryns and Mycenae, or to Stein in the deserts of Turkestan, to light upon the remains of a long-forgotten civilization. It looks, however, at this moment, as if we were on the threshold of such a discovery in the plains of the Indus.

Up to the present our knowledge of Indian antiquities has carried us back hardly further than the third century before Christ. Of the long ages before the coming of the Greeks and the rise of the Maurya dynasty; of the birth and growth of civilization in the great river basins; of the cultural development of the races who one after another poured into the peninsula from the north and west—of these and other problems relating to that dim and remote past, archaeology has given us but the faintest glimmerings; for almost the only remains of those early times that have come down to us have been rough implements of the Stone and Copper Ages, groups of prehistoric graves in the south of the peninsula, and some rude cyclopean walls at Rajagriha in Bihar. On the other hand, from the third century B.C. onwards, we have, on the whole, a fairly clear idea of man's handiwork in general: of his religious and domestic architecture, of his formative arts, of his weapons and utensils, of his personal ornaments and his jewellery, his coins and gems, and of the scripts which he used in his writing. And whenever it happens that new antiquities come to light—no matter to what race or religion they may belong—it is invariably possible to assign them with confidence and within relatively narrow limits to their respective age or class.

Now, however, there has unexpectedly been unearthed, in the south of the Panjab and in Sind, an entirely new class of objects which have nothing in common with those previously known to us, and which are unaccompanied by any data that might have helped to establish their age and origin.

The two sites where these somewhat startling remains have been discovered are some 400 miles apart—the one being at Harappa in the Montgomery District of the Panjab; the other at Mohenjo-Daro, in the Larkana District of Sind. At both these places there is a vast expanse of artificial mounds, evidently covering the remains of once flourishing cities, which, to judge from the mass of accumulated débris, rising as high as 60 ft. above the level of the plain, must have been in existence for many hundreds of years. Such groups of mounds abound in the plains of the Indus, just as they do in Mesopotamia and the valley of the Nile; and they are specially conspicuous along the banks of the old, dried-up beds of the main stream and its tributaries, not only in Sind, but in Bahawalpur State and in the Panjab.

The opportunities for excavation, therefore, in this part of India may be regarded as almost limitless; and, when it can be carried out on thorough and systematic lines, there is no doubt that the field will prove a peculiarly fertile one. Up to date, however, the meagre resources at the disposal of the Archaeological Department have permitted it to undertake little more than preliminary trial-digging on these two sites: and it goes without saying that the remains disclosed are correspondingly limited. Yet, such as they are, they are full of promise.

At Mohenjo-Daro, the main street of the old city can still be discerned as a broad highway running from the south bank of the river towards the south-east, with houses fringing it on either side. What is surmised by the discoverer, Mr. Banerji, to have been the royal

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palace, stood at the point where this road emerged on to the quays of the river side. Opposite to it, in the now dry bed of the river, are several islands from which rose the principal shrines of the city, the highest and, no doubt, the chief of them all, being a massive Buddhist *stupa* raised on a high oblong platform, and surrounded by subsidiary shrines and monastic quarters. These remains belong to about the second century A.D., when the Kushans were paramount in the north-west of India; and, judging by the finds already made—particularly the urn burials, remnants of painted frescoes inscribed in Brahmi and Kharoshthi characters, new types of coins and other novel objects—there can be no doubt that their further exploration will result in welcome light being thrown on this very obscure period of Indian history.

Valuable, however, as these remains are likely to prove, it is not in them that the real interest of Mohenjo-Daro centres at the moment. Deep down below the Buddhist monuments described above, or at other parts of the site appearing close to the surface itself, there are at least two other strata of buildings belonging to much earlier epochs, and containing a variety of brick structures—the character and antiquity of which can at present only be surmised. Among these older structures one group is especially worthy of mention. Besides various halls and passages and chambers, it includes a massive structure—apparently a shrine—with walls seven or eight feet thick, pierced by several conduits which, in the opinion of the excavator, served for carrying off the lustral water when the shrine or image within it was washed. In another part of the same group is what appears to be an altar built of small glazed bricks, and provided with a drain of similar brickwork. Some idea of the appearance of these early buildings, and of their present state of preservation, is afforded by two of the photographs reproduced (Plate I), the one showing a staircase to the south-west of the shrine referred to, with a conduit in the foreground from which the covering of marble slabs has been removed; the other, illustrating the glazed-brick flooring in a bay on the western facade of the same shrine.

At Harappa, Mr. Daya Ram Sahni's excavations disclosed as many as seven or eight successive levels, demonstrating the long and continuous occupation of the site during many hundreds of years prior to the third century B.C.; and throughout most, if not all, of this long period, burnt brick of a good quality was used for building purposes. The site at Harappa, however, has suffered much from the depredations of railway contractors and others, and the structures brought to light are in a more fragmentary condition than at Mohenjo-Daro. On the other hand, the smaller antiquities are generally identical in character with those from Mohenjo-Daro, and some of them even are better preserved. These smaller antiquities from the two sites comprise new varieties of potteries both painted and plain, some fashioned by hand and some turned on the wheel; terracottas; toys; bangles of blue glass, paste and shell; new types of coins or tokens; knives and cores of chert; dice and chessmen; a remarkable series of stone rings; and, most important of all, a number of engraved and inscribed seals. Iron does not occur at all, except in the latest deposits, and metal objects of any kind are scarce, particularly at Harappa.

Of all these antiquities the most valuable are the stone seals, not only because they are inscribed with legends in an unknown pictographic script, but because the figures engraved on them, and the style of the engraving, are different from anything of the kind hitherto met with in Indian art. Some of them are of steatite, others of ivory, and others of stone and paste. In shape most are square, and provided at the back with a boss pierced with a small hole for suspension. The animals engraved on them are in some instances bulls; in others unicorns; but it is to be observed that neither the Indian humped bull nor the water-buffalo occurs among them.

As to the strange pictographs which do duty for letters, three points are worthy of remark: first, that the marks (apparently vowel signs) attached to many of the pictographs indicate a relatively high stage of development; secondly, that some of the inscriptions from Mohenjo-Daro betray a later stage in the evolution of this script than those from Harappa; thirdly, that they bear no resemblance whatever to any ancient Indian alphabet known to us; but, on the other hand, they do bear a certain general affinity to pictographs of the Mycenaean age in the Mediterranean area, though it is not possible to point to any of the symbols as being actually identical.

Examples of the pictographic writing are found not only on the seal dies, but also (at Mohenjo-Daro) on certain oblong bars of copper which their discoverer assumes to have been coins, since they are similar in shape to the early Indian oblong coins known as "punch-marked," though they do not correspond in weight with any recognized standards used in ancient India. Should this assumption of Mr. Banerji's prove

correct, it would mean that these coins may turn out to be the earliest in existence, since the first coins hitherto known to have been struck in any other country are the Lydian pieces of the seventh century B.C.

Notwithstanding that the curious ring stones mentioned above have been found in large numbers on both sites, the purpose to which they were put has hitherto quite baffled the ingenuity of the excavators; though, for reasons into which it would take too long to enter here, Mr. Banerji believes that they were in some way connected with the *Bhartaris*, or shrines of eternal fire. They are of all sizes, from that of a small napkin ring up to fifty pounds in weight, and are made of various coloured stones or marble; but what is particularly curious about them is that in many specimens the upper and lower surfaces are undulating.

Another remarkable and significant feature at the Mohenjo-Daro site is the character of the burial customs. In the earliest period the practice was to bury the body in a hunched position in a brick tomb, generally of square or oblong form. Later on (it may be very much later), the custom obtained of burning the body, as is commonly done in India to-day, and depositing the ashes in a small urn, which, along with two or three others, was placed inside a larger round jar, accompanied by several miniature vessels containing food, raiment, and so on.

To what age and to what people do these novel antiquities belong? Those are the two questions which will naturally occur to the reader, and to which a score of different answers may perhaps suggest themselves. As to the first question, all that can be said at present is that the period during which this culture flourished in the Indus valley must have extended over many centuries, and that it came to an end before the rise of the Maurya power in the third century B.C. So much may be inferred, on the one hand, from the many successive strata of habitation, particularly on the Harappa site: on the other, from the presence of copper weapons, and the total absence of any iron on either site, as well as from the fact that none of the objects, except the bricks and a few toy terracottas, can be paralleled among the known antiquities of the Mauryan or subsequent epochs; while the pictographic writing is totally distinct from the early Brahmi script which the Emperor Asoka employed throughout the greater part of India, or from the Kharoshthi script which he used in his inscriptions on the North-Western Frontier.

As to the second question, it is possible, though unlikely, that this civilization of the Indus valley was an intrusive civilization emanating from further west. Painted pottery and other objects somewhat analogous to those from Mohenjo-Daro and Harappa have been found in Baluchistan; and there are linguistic reasons for believing that it was by way of Baluchistan that the Dravidian races (thought by some writers to have been originally connected with the Mediterranean) entered India. Mr. Banerji himself is inclined to connect this culture of the Indus valley directly with the Aegean culture of the Eastern Mediterranean, and holds that distinct affinities are traceable between the Minoan antiquities of the Cret, and those unearthed by him at Mohenjo-Daro—especially in regard to the painted ceramic wares and pictographic inscriptions. But the resemblances referred to are, at the best, problematical, and, in any case, too slight and intangible to warrant any inference being drawn as to a cultural connection between the two areas.

What seems *prima facie* more probable is that this forgotten civilization, of which the excavations of Harappa and Mohenjo-Daro have now given us a first glimpse, was developed in the Indus valley itself, and just as distinctive of that region as the civilization of the Pharaohs was distinctive of the Nile. In the marvellous forward progress which mankind made during the Neolithic, Copper, and Bronze Ages, the great river tracts of the then inhabited parts of the world played a most important part; for it was in these tracts that conditions were found most favourable for supporting a dense and settled population—namely, fertility of the soil, an unfailing water supply, and easy communications; and it was, of course, among such large and settled populations that civilization had the best chance of making progress. The debt which, in the early stages of its development, the human race owed to the Nile, to the Danube, to the Tigris, and to the Euphrates, is already well known. But how much it owed to the Indus and to the Ganges has yet to be determined. In the case of the Indus, it is probably true that successive migrations from outside had a useful effect, as they did in Mesopotamia and in Egypt, in promoting the development of indigenous culture; but there is no reason to assume that the culture of this region was imported from other lands, or that its character was profoundly modified by outside influences (See Plates I, II and III).

Remarkable Discoveries in India

A.H. SAYCE

The remarkable discoveries in the Panjab and Sind, of which Sir John Marshall has given an account in *The Illustrated London News*, September 20, are even more remarkable and startling than he supposes. The inscribed "seals" or plaques found at Harappa and Mohenjo-Daro are practically identical with the Proto-Elamite "tablettes de comptabilité" discovered by De Morgan at Susa. The form and size of the plaques are the same, the "unicorns" are the same, and the pictographs and numerals are also the same. The identity is such that the "seals" and tablets might have come from the same hand. The tablets, which are very numerous, have been published by Scheil in the "Mémoires de la Mission Archéologique de Perse," VI. (1905) and XVII. (1923). They belong to the third millennium B.C., and extend from the age of the Babylonian King Manistusu to that of the Third Dynasty of Ur. A native king a little later has added a text in the same pictographs to a cuneiform inscription. It is evident, therefore, that as far back as the third millennium B.C. there was intercourse between Susa and the North-West of India. The discovery opens up a new historical vista, and is likely to revolutionize our ideas of the age and origin of Indian civilization. So far as I can gather from the description of the painted pottery accompanying the plaques, it resembles that of the Susian "second style," which was contemporaneous with the tablets. An inscribed "seal" from Harappa was published by Terrien de la Couperie in an early number of the "Proceedings of the Society of Biblical Archaeology."

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The New Links between Indian and Babylonian Civilizations

C.J. GADD and SIDNEY SMITH

The close resemblance of the objects from Mohenjo-Daro and Harappa, in the Indus valley (described by Sir John Marshall in *The Illustrated London News* of 20 September), to Babylonian antiquities, which has been noted by Professor Sayce, is very striking when pictures of various classes of object are seen side by side. It is, therefore, worth examining the nature of the similarity.

The most important objects in this respect are the seals from Harappa, which appear to be exactly similar in shape to square stamp seals which are commonly found at Susa and on Babylonian sites of the early period. The bulls are distinctly Sumerian in appearance, especially in the details of the horn, the neck, and the fold of flesh from the foreleg to the shoulder. Again, the bull stands before a cult object, which may represent something in a large pot, or perhaps a sheaf beside a large pot; this theme is a familiar subject in Babylonian art of all periods, and, if the object be a sheaf, it very closely resembles the Sumerian picture sign *ZAG*. As to the signs on the Harappa seals, which obviously form some kind of writing, the illustrations will sufficiently show that nine of these signs very closely, and seven partially, resemble Sumerian writing; while many of the others may prove to be related. Above all, the numeration system appears to be the same; a very striking case is the number ||| ⟨ || on one Indian seal which is very similar to the Sumerian 𒐈 𒌋 𒐖 $= 3 \times 60 + 10 + 2 = 192$. These close resemblances of the Harappa seals to the products of Babylonian civilization cannot be accidental. The people who made these seals must have been in very close contact with Sumerian civilization, and have borrowed their artistic style and the basis of their writing from the Sumerians (see Figs. 11.1 and 11.2).

This conclusion is reinforced by consideration of the other objects. Thus the cylindrical piece of haematite with flattened ends from Mohenjo-Daro is an exact counterpart of a common type of weight commonly used in Babylonia for lighter objects till the end of the third millennium. The stone objects thought to be "shrines of fire" by Indian archaeologists can most aptly be compared to Babylonian mace-heads, which vary in size and weight exactly as the Indian objects are said to do, and are of exactly the same form. These mace-heads would be votive offerings in a temple precinct—one more close parallel between the Indian civilization and the Babylonian. The pieces of shell inlay could be paralleled by many examples from Southern Babylonia—indeed, the fondness of the Sumerians for shell, both plain and decorated with incised patterns, is a distinctive feature of their civilization. The clay figurines are, of course, of a type common in many countries; but it is noteworthy that the scroll head-dress on one from Harappa very closely resembles the symbol of a Babylonian goddess. The small figurine of a cock from Mohenjo-Daro may be paralleled from Ur, and the bird is pictured on Babylonian boundary stones of the second millennium. It would be very interesting to know the weights of the bars of copper; possibly they would fit in with the Babylonian system used all over Western Asia.

The last and not the least important similarity is to be found in the type of brick building with a drainage system and ornamentation of glazed bricks. On looking at the pictures of the Indian excavations (see *The Illustrated London News* of 20 September, and examples given on page 616 in the present number), one involuntarily calls to mind the pictures of Dr. Hall's and Mr. Woolley's excavations at Ur of the Chaldees

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INDIAN SIGN	SUMERIAN SIGN	APPROXIMATE DATE OF USE	PHONETIC VALUE	PICTURE VALUE
		2750	BAR	a kind of shrine.
		3000.	GI.	reed.
		3000	GAN.	A land measure.
		3500	MAL. GA.	a dwelling
		3000	—	—
		2400.	GIL	(a doubled form of GI above)
		3000	GIR. AD.	a scorpion

FIGURE 11.1. *Indian and Sumerian signs which resemble one another in general form. (Editor's note: These parallels and those in Fig. 11.2 are no longer thought to be of significance).*

already published in *The Illustrated London News* (see numbers for March 17, April 23, and July 28, 1923, and April 1, 1921). An important point will consist in a comparison of the measurement of the Indian bricks (which may possibly conform to Babylonian measures) and in the type of firing, which in Babylonian bricks is distinctive. The date of the Indian buildings will not affect the validity of this comparison, for Babylonian building styles from 2700 B.C. onwards changed very little.

All the resemblances yet noted are consonant with the view that the people who made these objects at Mohenjo-Daro and Harappa were in close contact with the Sumerians between 3000 and 2800 B.C. The pottery presents more difficult problems. The only painted pottery found in Babylonia considerably antedates 3000 B.C., as does also the Susian pottery. On the other hand, the painted pottery of Babylonia and Susa definitely belongs to a period when metal was only just beginning to be used, which seems to accord with the evidence from the Indian sites. Again, the miniature funeral pottery from Mohenjo-Daro is strikingly similar to miniature pottery found at Ur which seems to belong to the second millennium and even later. It may be, therefore, that pottery of widely different dates is found together on the Indian sites; but there would be nothing really surprising in the discovery that this pottery, whether painted or of the plain miniature type, was a reminder of a trade between India and Iraq by way of Beluchistan.

The general trend of discovery for some time past has been to show a close connection between an Indian race of Aryan extraction and Mesopotamia. Thus, somewhere between 1400-1200 B.C. some scribe wrote a tablet in a Mesopotamian language concerning horse-training which employs words for the numerals that closely resemble the Sanskrit. About the same time Indra Varuna and the twins were worshipped in Mesopotamia. Whether the fact that results from the new archaeological finds—namely, that there were in India a people who had been in close contact with the Sumerians—should be connected with the existence of this Aryan race in Mesopotamia, or whether the earlier contact belongs to a separate and distinguishable race, we must await further discoveries to decide (see Plates IV and V).

FIGURE 11.2. *Evidence of early intercourse (hitherto unknown) between the two civilizations.*

INDIAN SIGN	SUMERIAN SIGN	APPROXIMATE DATE OF USE	PHONETIC VALUE	PICTURE VALUE
		2800 -2700	KHA	Fish
		2400.	SAR.	360
		3,000	GAL.	great
		3000	SAG.	heart. in
		2800 2400.	BAD	dead
		3000	KU ŠU	to
		2800.	ŠU.	hand.
		2800	UŠ.	membrum virile
		3000	E	house, plot of land.

Part IV Contacts with the West: Chronological Considerations

Editor's Introduction

It has long been known that there is a small body of artifacts which are indicative of contact between the Indus and Mesopotamian Civilizations. Some of these were noted by Marshall, Gadd and Smith in 1924. The papers in this part expand and analyze these finds. By the early 1930s, when the two articles by Gadd and Mackay were published, the pattern of finds was largely complete, although further excavations in both areas have added more numbers to the already enumerated artifact types. An exception to this is the Persian Gulf seal from Lothal described by S.R. Rao.

The seals of Indian style found in various Mesopotamian sites are undoubtedly the most important artifacts indicating contact between the two civilizations. But it should be kept in mind that in this case we are dealing with a style and many of these seals are not typically Harappan. In fact, the number of undeniably Indus seals from Mesopotamian sites is very small and almost universally found in poor contexts. But the recently discovered seal from the Kassite levels of Nippur is both reliably placed stratigraphically and of purely Indus type with script the so-called "unicorn" bull standing before a "manger" (Macguire Gibson 1975:personal communication). In some instances seals have been placed in the "Indus" category simply because they do not appear to be of Mesopotamian inspiration and may contain a motif or device, such as a humped bull or scorpion, which some believe to indicate Indus influence. Even if these attributes are correctly assigned, objects of Indus style are significantly different from objects of Indus origin in terms of the conclusions which we might draw concerning the cultural relationships. The seals are, however, useful in comparative chronology and this has been critically assessed by Wheeler (1968:114-8).

In the papers by Dales and Lamberg-Karlovsky attention is called to the considerable disparity in the number of items which can be compared between the Indus Valley and Mesopotamia. There are a number of objects in Mesopotamia for which a closely argued case could be made for direct import from the Indus Valley. Other types frequently mentioned in this regard are either doubtful comparisons or derived from a third source by both civilizations. The latter is certainly the case for the carved steatite vessels noted by Lamberg-Karlovsky. On the other hand, there is only one find from an Harappan site which must come from the west: the Persian Gulf seal from Lothal. Indus sites have not produced cuneiform documents or inscriptions, ceramics, or any of the objects which form the corpus of Mesopotamian small finds. Such a disparity in data must remain in the forefront of our thoughts when addressing the problem of cultural inter-relations between these civilizations. In the end, serious sampling error aside, this disparity is indicative of a particular pattern of interaction and statements which seek to explain the relationship must be congruent with this aspect of the data.

Dales and Lamberg-Karlovsky both attempt to characterize the nature of this interaction, and use essentially the same body of data. Some differences emerge in their conclusions, yet both seem to make sense. This is a testimony to the current "state of the art" in these matters and reflects not only a genuine poverty of well ordered data but also the crudeness of our interpretations. We do not understand the mechanisms of this interaction, or the range of goods which accompanied it. Nor do we know who participated in these arrangements. Was it a case of direct contact by the parties involved, or were goods moved through intermediaries, possibly many of them along a chain of relationships? There is no way to judge the

true volume or scale of material exchange. Was it high volume, economically oriented commerce in raw materials and/or finished products intended for mass consumption at the village level? Was it essentially a kind of noneconomic, symbolic exchange between elites which served to order and validate other kinds of sociocultural relationships? Did the goods move by sea, overland, or some combination of these routes? Answers to these and other questions are still left unanswered. For the moment it seems that raw materials may have played an important role in the exchange. This would at least account for the paucity of finds, especially in India. That the inhabitants of Mesopotamia and the Indus Valley frequently travelled directly to one another's lands seem unlikely since the economic records which were important to Mesopotamian if not Harappan commerce, have yet to appear in "foreign" contexts.

We are thus left with the impression that the trade was indirect and that relatively few finished products were involved. Of course, if this small number of finished items was the substance of the trade, we would be dealing with a minimum of economic impact. This in turn would suggest that the trade was directed to a small segment of each society—the elites, probably—and that the trade had a highly symbolic aspect. A test for this would be possible if we assume that the elites were concentrated in the cities and that the imported items ought not to be found in the smaller, village size settlements.

The reader may already have questioned the absence of reference to perishable goods such as cloth, leather, oils or even food, which may have been a part of this commerce and would have had an effect on the very tentative direction toward some conclusions. This has been done purposefully since even products such as these would leave some kind of record. Perishables would have been packed and sealed. Liquids would have been transported in pottery vessels. Clothing, even leather goods, often have sewn decorations which would survive. Thus, even if the goods themselves were perishable it is still reasonable for us to expect an archaeological record of such trade.

The trade may have been by land, by sea or some combination. Based on Lamberg-Karlovsky's excavations at Tepe Yahya we know that there was some, apparently important, overland commerce on the Iranian plateau in the third millennium. The sea trade is another question and will be dealt with in the next part of this book.

Seals of Ancient Indian Style Found at Ur

C.J. GADD

The very great interest which has, in the last few years, been attracted to the excavations of two early sites, Mohenjo-Daro and Harappa, in the valley of the Indus, was first aroused by Sir John Marshall's revelation of his discoveries in the autumn of 1924. Notable similarities to various Mesopotamian antiquities were not long in being observed and pointed out, but the most remarkable of all the objects found, the seals engraved with animal-figures and inscriptions in a completely unknown character, were not in themselves comparable with Mesopotamian seals, even if the new signs were occasionally reminiscent of the early Sumerian pictographs. It is not a little surprising, therefore, that in the preceding autumn (1923) a specimen of these Indian seals had been found by a scientific expedition[1] working at the site of the ancient city of Kish, near the Euphrates. With this evidence alone it became manifest that the cultural similarities between the two regions must have depended upon a definite contact in historical times; at what period could not be inferred from the position in which the seal was found at Kish, beyond the assurance that it was at least before 2000 B.C. Early in 1924 a seal of the same type was purchased by the Louvre, and this was said to have been found in the ruins of Lagash, a celebrated site in southern Babylonia. In publishing this acquisition M. Thureau-Dangin[2] observed that the attribution to Lagash might well be justified in view of the authenticated discovery at Kish, and also drew attention to the presence of two more seals of this kind in the collection of the Louvre, which were known to have been found at Lagash and Susa respectively. Meantime an imprint of such a seal upon the fragment of a clay label from a bale of cloth had also been published[3] by Father Scheil, and this was said to come from the site of Umma, the neighbour city of Lagash. Since that time another example has been recovered from Lagash in the resumed excavations there,[4] and recently others have come to light at Kish[5] and at Tell Asmar (Ashnunnak).[6] Two unconnected specimens are of very recent revelation, both having been obtained from dealers; one in the possession of Professor A. B. Cook of Cambridge,[7] and a second (no. 18 in this paper), with unusually interesting features, very lately given to the British Museum.[8] Other isolated examples which have been illustrated—one belonging to Baron von Bissing[9] and one in the Boston Museum of Fine Arts[10]—may belong to the original find[11] which first brought these seals to Europe, three of them into the Department of Ethnography in the British Museum.

At the moment of writing, therefore, there have been seven seals and one imprint of the Indus style identified as coming from Mesopotamian sites (if Susa may be included, for convenience), as well as two others mentioned above of undefined origin but almost certainly from Babylonia. A considerable addition to this number is made by the specimens found at various times and places in the excavations at Ur, which I am enabled to present here by the kind permission of the Directors of the British Museum and of the University Museum of Pennsylvania. These seals must, of course, be compared with the much more numerous "native" specimens recently published in so splendid a manner by Sir John Marshall and his collaborators in *Mohenjo-Daro and the Indus Civilization*, a work which will be represented hereinafter by the initial M.

No. 1. First among the seals discovered at Ur is the

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unique object (Plate VI, no. 1), formerly illustrated and described by Mr. C. L. Woolley in the *Antiquaries Journal*, vol. viii, pl. XI. 2, and p. 26, which is now in the British Museum.[12] It measures $1\frac{1}{16} \times \frac{15}{16} \times \frac{7}{16}$ in., is made of grey steatite, and on the back is shaped into a ridge perforated lengthwise, thus belonging to the class of "button" seals, which form is characteristic of the Indus style.[13] On the face stands, below, the figure of a bull with head bent down, as on certain Indian examples,[14] but there is no trace of the manger (?) in front of the bull which is found at Harappa and Mohenjo-Daro, the horns are shown in full development, not in the "unicorn" convention, and there is a narrow band thrown over the animal midway along the back.[15] The figure is divided from the inscription above by a horizontal line, which never appears with the unknown script. It is, however, the inscription itself which is the main point of interest, for it is in archaic cuneiform writing. It is always hazardous to date an object solely by the epigraphy of a cuneiform inscription, especially in the case of seals, and here, therefore, it is well not to attempt greater exactitude than is conveyed by the statement that these signs are in any case pre-Sargonic, that is, of a period before 2500 B.C. There are three signs and very probably traces of a fourth, almost obliterated; the three preserved are themselves scratchy and rather worn, though not ill-formed. Hence their reading is doubtful—the choices are, for the first SAG(K) or KA, for the second KU or possibly LU, while the third is almost certainly ŠI, and the fourth, if it existed at all, is quite uncertain. To all this uncertainty is also to be added, of course, the polyphony of the cuneiform signs, and the result is that the total number of possible readings would be considerable. Nevertheless, using the commonest values of the signs, *sak-ku-ši*—(with possible loss of something at the end) may be pronounced the best provisional reading. Unfortunately, as the cuneiform stands alone it helps nothing towards the reading of the Indus script, and upon the possible word thus obtained it would be idle to speculate. It does not, at least, seem to be any Sumerian or Akkadian name. Not only the inscription but the style of the object and of the carving make it unlikely that this seal should be ascribed to the Indus valley. It is either a local imitation, made at Ur, of a foreign type, or, more probably, a product of some place under the influence both of the Indus and of the Sumerian civilizations. Mr. Woolley has already stated that there is no evidence to be gained as to the date of this object from the circumstances in which it was found.

No. 2. Next to be described is the subject of Plate VI, no. 2. This is a circular stamp-seal, only slightly broken on one side; diam. 1 in., height $\frac{9}{16}$ in.; now in the British Museum, no. 122187. It was obtained at Ur in the season 1928-9, but there is no evidence of where it was found. The material is the regular light, flaky steatite of the Indus seals, with the highly-glazed white surface that looks like enamel, the back has a pronounced "button" boss, pierced in one direction and divided across the other by a groove, as the illustration shows.[16] On the face is the short-horned bull figure with lowered head, the legs being mostly obliterated, and above is an inscription of five characters, which are adequately shown by the illustration; the first (over the bull's head) is . In material, style, and inscription, therefore, this object might be of native Indian production; only the circular shape[17] is decidedly unusual at Mohenjo-Daro and Harappa.

No. 3. The subject of Plate VI, no. 3, was found at Ur in the season 1930-1 but again without archaeological context: it is now in the British Museum, 122946 (excavation number U. 17342). Measurements, $1 \times \frac{5}{8}$ in. Not quite half is preserved, but enough to show that it was a stamp-seal of the "button" type, and that it bore an Indian inscription the remains of which are clearly seen in the photograph. Its material is a greenish-grey steatite, not so flaky in appearance as usual, and only suggestions of the creamy surface remain.

No. 4 was found at Ur in the same year as the preceding. It is now in the British Museum, no. 122188, and measures $\frac{7}{8} \times \frac{11}{16}$ in. (Plate VI, no. 4). This is a mere fragment, more of the back than of the face being preserved, but in every respect it is similar to the foregoing more complete example, and had an Indian inscription of which only the "fish" sign and a fragment of another are preserved.

No. 5. Of the next (Plate VI, no. 5) the lower half of the face alone survives. This fragment was found at Ur in the season 1930-1; excavation number U. 17341, now in the University Museum of Pennsylvania, dimensions $1 \times \frac{1}{2}$ in. It appears to have come from the mostly unexplored area called Diqdiqqah, outside the city, where many small finds are made by the workmen and their families. As the illustration shows, it resembles the foregoing seals in every way, the material of this one being a brownish grey steatite turning to the familiar creamy colour towards the top. The animal has a tail with a prominently-marked "point".

Nos. 6, 7. These may be considered together, for, in

contrast with all the others, they are not stamp-seals but cylinders, a type which is hardly known at Mohenjo-Daro[18] and Harappa, but is of course characteristic of Babylonia. No. 6 (B.M. 122947, 1 in. × $\frac{5}{8}$ in.) is a cylinder of some white stone, perforated in the usual way, and having at each end grooves with the outer edges nicked, probably to be fitted with metal caps, as we see also upon those found at Susa and at Tell Asmar. No. 6 came from Ur in the season 1930-1, having been discovered in a vaulted tomb which is apparently that described[19] by Mr. Woolley as "a Larsa tomb which had been hacked down into" a wall dividing two apartments in the "N.W. annexe" added by Bur-Sin, king of Ur, to the funerary building of his father. The design shows a palm-tree, before which stands a humped bull, feeding from a round manger, or perhaps a bundle of fodder is intended.[20] Behind the bull is a scorpion and two snakes, and above the whole a human figure, placed horizontally, with fantastically long arms and legs, and rays about his head. The style of the bull, and the object before him, are evidently in close relation with the like representations on the Indus seals, and the strange human figure would be hard to parallel on Babylonian seals: this example is evidently Indian or under strong Indian influence. The same is true of Plate VI, no. 7 (excavation number U. 11958), of which only a photograph is at present accessible. A bull, unhumped, of the so-called "unicorn" type, raises his head towards a simplified version of a tree, and two uncertain objects, one a sort of trefoil,[21] are shown above his back. Under his head is an unmistakable character of the Indus script, the "fish" with cross-hatchings.

Plate II shows six round stamp-seals, of a remarkably uniform style, though all differing completely in the devices which they bear. No. 11 is the most like in its shape to Plate VI, 2 and 4, and to the Mohenjo-Daro round seals (M. III. CII. *k, l*), for it has at the back a central boss rising to an unusually high cone, and a groove across the middle of it, but differs in adding the four small circles with centre-spots which will be observed on all the others shown in this plate. different from the "Indian" type. The bosses are very large, occupying all but a narrow margin of the back, and rise only to a moderate height. There is the usual perforation made to slope downwards from the two ends,[22] but the bosses are divided (in all but no. 11) by a treble or double groove, and there are the characteristic circles already noticed. Following are the descriptions of the several examples.

No. 8 (B.M. 118704, excavator's number U. 6020; diam. $\frac{13}{16}$ in., ht. $\frac{7}{16}$ in.). This seal is of light grey steatite with the usual creamy surface, worn away in places and thus exposing the grey underneath. The scene represented appears to be one of sacrifice; two figures carry between them a vase, and one presents a goat-like animal which he holds by the neck. In both the kinds of creatures represented this seal is quite unlike those from Mohenjo-Daro. The goat has no resemblance to the "antelope" which may be depicted in M. III, pl. CX, nos. 302, 303, and the human figures none to the demons or men of nos. 356-8, nor to the statuettes and figurines of M. III, pls. XCIV, XCV, XCVIII-C. On the other hand, the skirts marked with three rows of vertical strokes have an unmistakable likeness to the characteristic early Sumerian garment of fleece, and the style of delineating the heads and the attitude of the goat would be entirely normal as Sumerian. Indeed, the interest of this seal resides in its combination of a Sumerian device with form and material which are no less clearly those of the Indus civilization.[23] The object was found at Ur in the season 1925-6, loose in the soil close to the surface in the area[24] designated as EH.

No. 9 (B.M. 122945, excavator's number U. 16181; diam. $\frac{7}{8}$ in., ht. $\frac{7}{16}$ in.). Found at Ur, without context, in the season 1930-1, this is a good deal worn and partly broken both on front and back. It is again of a greenish-grey steatite, exposed over most of the surface by the detrition of the glaze, of which, however, a good deal still remains, of the usual substance and colour. The engraved face is divided into four quadrants by lines, each of which terminates at the edge of the seal in some elaborately shaped object which is most probably a vase out of which the dividing lines spring. Each of the quadrants (though one is mostly destroyed) is occupied by a naked figure, sitting so that, following round the circle, the head of one is placed nearest to the feet of the preceding. Two of these figures (in alternate quadrants) clasp their hands upon their breasts, the other (as probably also the fourth) spreads out the arms, making a beckoning gesture with one hand.

No. 10 (B.M. 120576, excavator's number U. 9265; diam. $\frac{11}{16}$ in., ht. $\frac{3}{8}$ in.). Found at Ur, without context, in the season 1926-7. This seal is also of steatite and has the usual features, as may be seen in the plate. The device is puzzling; there is, below, a bull with long horns roughly depicted, but above is a rather uncertain addition, which is perhaps an attempt to show one (possibly two) more, in a couching position, as viewed

by turning the seal round until the face of the standing bull is downwards. If this is intended, the head of the second bull is turned back, and it is not, perhaps, quite impossible that the remaining part of the design is meant for a bird, such as is fairly often seen perched upon the back of a bull in Sumerian art, a device which has not yet been certainly explained.[25] Its occurrence here is far too doubtful to be dwelt upon.

No. 11 (University Museum of Pennsylvania, excavator's number U. 16397; diam. $\frac{11}{16}$ in., ht. $\frac{7}{16}$ in.). Found at Ur, without context, in the season 1930-1. As already observed, this seal has an unusually high boss and a single groove across it. Otherwise it does not differ, in material or style, from the rest. The device is notable, a scorpion and an eye (?). The latter (or at least a similar elliptical character) appears in the script (M, pl. CXXI) both alone and with a number of modifications. The scorpion is, however, of doubtful occurrence in the script. Mr. Mackay suggests (M. II, p. 392) that the fairly common character (ibid., pl. CXXVIII, no. CCCLI) "may be a scorpion," but it seems to me very improbable, and among the few signs which at least may represent creeping things, it is not possible to identify the scorpion. Nevertheless this seal strongly suggests writing, an idea which is reinforced by no. 12 on the same plate, and by the analogy of the early Minoan "hieroglyphic" seals. If so, this is an example of the "Indus" script in an unconventionalized form, and thereby of great interest. But the same thing is more notably represented in the next example, to which we proceed.

No. 12 (University Museum of Pennsylvania, excavator's number U. 16747; diam. 1 in., ht. $\frac{5}{16}$ in.). Found at Ur in the season 1930-1, "from upper rubbish, Kassite (?) level," over one of the houses[26] in the domestic quarter. The seal itself, as may be seen from the illustration, is of the same type as the others, but its device is of peculiar interest, the central figure being that of a water-carrier, with a skin (or pot?) hung on each end of the yoke across his shoulders and another one below the crook of his left arm. The vessel hanging from the right end of his yoke is over some object which looks like a stand with high sides, but may represent a receptacle for the water. On either side of the man's head is a star, and the whole subject is enclosed by "parenthesis" marks. This is, of course, an unmistakable example of an "hieroglyphic" seal, a legend in the form of fully-developed pictures, for the water-carrier with his yoke and two skins (?) is one of the most identifiable figures in the ordinary script (see M, pl. CXXIX, nos. CCCLXXXVI ff.), the object under the man's left arm appears as CLXXVIII in the same list, the "pot-stand" is probably CCCXVII, the "parenthesis" marks are XX; the stars are not obviously represented in the Sign-Manual. The object most closely comparable with this seal is the fragment of an ivory plaque from Mohenjo-Daro (M, vol. iii, pl.CXXXII, no. 10, cf. vol. ii, p. 562 f. and 408) which shows a man *plene scriptus*, as it were, for the last character listed in the Sign-Manual. Among the seals, however, whether from India or Mesopotamia, the present example is unique in character, with the possible exception, as suggested above, of the preceding number.

No. 13 (B.M. 122841, diam. $\frac{15}{16}$ in., ht. $\frac{3}{8}$ in.). Found at Ur in the season 1929-30, without context. Materially the seal is of the usual type. The face is quartered by four forked branches springing from the angles of a small square, with some internal decoration, in the middle. In each of the four spaces thus formed is an uncertain decoration, which might be an animal head, except for the circle with centre-spot which can be seen in them. The device seems purely decorative.

No. 14 (Plate VIII, University Museum of Pennsylvania, CBS. 16301, excavation number U. 7027, diam. 1 in., ht. $\frac{7}{16}$ in.). This seal, and the next to be mentioned, have been published by Dr. Legrain in the Philadelphia *Museum Journal* for 1929; this one is figured on pl. XXXVIII as no. 94 and is described on the opposite page. Dr. Legrain has very kindly supplied me with the photograph that I reproduce here showing the back of the seal, which appears to be entirely similar to the foregoing specimens. The Sumerian character of the device is strongly marked, especially in the ithyphallic bull-men, the so-called "Enkidu" figure which is extremely common upon Babylonian cylinders of the early period. Nor is there anything in the seated god that is un-Babylonian; he and the bull-men all have horned head-dresses, and the moon-symbols upon poles seem to represent the door-posts that the pair of "twin"[27] genii are commonly seen supporting on either side of a god. The bull standing underneath is more doubtful, but even this is not markedly Indian, since it does not wholly correspond with any of the Mohenjo-Daro types described in M, vol. ii, 382 ff. This, then, is a seal completely of the "Indus" type so far as material and shape are concerned, whereas the device is much rather Babylonian than Indian. In this respect it may be compared with no. 8.

No. 15 (University Museum of Pennsylvania, exca-

vation number U. 8685, diam. $\frac{15}{16}$ in.). Found at Ur in the season 1926-7, in the cemetery area, in a ruined grave 0.9 metres from the surface, together with a pair of gold ear-rings of the double-crescent type and long beads of steatite and carnelian, two of gilt copper, and others of lapis-lazuli, carnelian, and banded sard. These contents, and the small depth from the surface, suggest that this grave belonged to the Sargonic[28] series, and if this may be presumed, it is a point of importance for the dating of these objects, a subject that will be considered later. This seal has, like the preceding, been published and described in the *Museum Journal* for 1929, pl. XLI and p. 306, by Dr. Legrain. The inscription is crowded and not very distinct, the most remarkable sign being the first one to the left (in the impression) having the form of a flower or perhaps an animal's skin with curly tail. This does not seem to be found elsewhere, and the round spot upon the bull's back is also curious.

No. 16 is the last of the Ur series to be described. It was found in the excavations of 1931-2 (B.M. 123208, excavation number U. 17649, diam. $\frac{7}{8}$ in.). The context in this case was definite; it occurred in the filling of a tomb-shaft which Mr. Woolley ascribes[29] to the Second Dynasty of Ur. Here the bull is without a "manger" or anything under the head, and the four characters, though rather roughly shaped (indeed, the workmanship of this seal is poor throughout), are all well known. There are few traces of the customary "glaze" on the surface of the seal, but it can be plainly traced at the bottom of the carvings. The back, boss, and perforation are of the style of nos. 2-5.

The foregoing sixteen examples have all been found in or about the excavations at Ur in the last few years. As an appendix to them this is a convenient place to add two more seals of the same class, not connected with Ur, but almost certainly found at some other place in Babylonia, as to which there is no evidence.

No. 17. The first has long been in the Department of Egyptian and Assyrian Antiquities at the British Museum, and is numbered 120228: I am enabled to include this and the next seal by permission of the Keeper. It is of the usual glazed steatite, and originally had the pierced boss at the back but this is now broken away, leaving only the trace of the boring. The object is also a little damaged at the edges by the chipping off of the surface; its diameter is 1 in. There is, unfortunately, no information as to its origin.[30] The bull is of the short-horned variety and stands over the "manger" generally associated with this animal on the Mohenjo-Daro seals. The inscription is well preserved and can be plainly seen in the illustration; the signs are in the best Indian style.

No. 18. The other is a recent acquisition[31] of the same Department in the British Museum. It was noticed among a London dealer's stock, to which it had come from another dealer in Baghdad. There is, consequently, a presumption, as in the preceding instance, that the object was found somewhere in Babylonia. It is now numbered 123059, and has a diameter of $1\frac{1}{4}$ in. and height $\frac{3}{4}$ in., being therefore of unusual size. The material is steatite, almost black, but covered with the usual creamy glaze which is worn off at the edges. The illustration shows both the face of the original, and an impression, which makes the subject clearer; the back has the usual boss, with single groove, and perforation. On the face is an engraving somewhat damaged but still quite plain. Above there is an inscription of five characters, most prominent among them two "men" standing side by side. To the right of these (in the impression) is a damaged "fish" sign, and to the left two others which closely resemble CCXCVII and XXVIII of the Sign-Manual in M. Below is a unique representation of a bull in the act of mating with a cow; the head and legs of the cow are rather obscured by damage and the tuft at the end of the tail is summarily shaped like an arrow-head, but nevertheless the purport of the device is quite clear. As already observed it has not as yet been found on any other of these seals.

The individual seals having been thus described, it remains to say something upon two subjects, the shape and dating of these objects.

It will have been observed that what may be called the "Mesopotamian" seals of this class differ from the great majority of those found in India in two principal respects—the prevalence of the round instead of rectangular shape in the stamp-seals, and the much higher proportion of cylinders to stamps. To take the latter first: cylinders are hitherto a rarity among the Indian finds, only five[32] (and those not certainly seals) having appeared at Mohenjo-Daro. But among the much smaller total hitherto found in Babylonia there are already several cylinders, namely, from Susa (Delaporte, *Catalogue des cylindres etc. du Louvre*, pl. 25, no. 15), from Ur (nos. 6 and 7 in the present publication), and from Ashnunnak (Frankfort in *The Times*, 26 March 1932). This greater frequency of cylinders is perhaps to be expected in a land where the cylinder-seal was generally the standard, but these "Indus" seals display in general so completely foreign

a character that they might not be expected to have been influenced by the custom of the land in which they chance to be found. Their devices and inscriptions (when existing) are purely Indian. It would be of interest to compare the material of which they are made with that of the stamp-seals. The Susa cylinder is described as of "bone" by Delaporte in his *Catalogue*, p. 45 (s. 299); Dr. Frankfort has kindly told me that the Ashnunnak seal was of steatite, so far as he could judge by inspection. Of the two Ur specimens no. 7 (U. 11958) is at present inaccessible. The other, no. 6 (U.16220), is of a fairly hard white substance, which shows white even in fractures. I am not in a position to obtain an expert identification of this material, but Dr. Plenderleith, of the British Museum Laboratory, has informed me, after applying various tests, that he is disposed to think it is neither ivory, shell, nor steatite, but some harder kind of white stone. At present, therefore, it is not possible to obtain by means of the material any suggestion as to the origin of these pieces. As to the prevalence of the round shape over the rectangular—the direct opposite of what is found at Mohenjo-Daro—this is very marked[33] among the specimens at present recovered from Babylonia, but it is hardly possible to draw any inference from this fact save that the import into Babylonia probably did not come from Mohenjo-Daro or Harappa. But since it is evident that many other sites of the same civilization must have existed, and will doubtless in time be explored, this conclusion is of no great interest.

The amount of evidence which the discovery of these seals at Ur has furnished for the determining of their date is rather disappointing. None the less what indications they do give, as well as those which are being obtained on other sites, seem to corroborate the judgements which were expressed several years ago about the approximate date of the flourishing period of the "Indus" civilization. Among the seals from Ur published here there are four only which are described as having been found in possibly datable circumstances:

(*a*) No. 6, from "a Larsa tomb which had been hacked down into" a wall dividing two rooms in a building of the Third Dynasty of Ur.
(*b*) No. 12, from "upper rubbish, Kassite (?) level" over a house, presumably of the Larsa period, in the domestic quarter.
(*c*) No. 15, apparently in place in a burial indicated both by its contents and by its position as belonging to the Sargonic age.
(*d*) No. 16, found in the filling of a tomb-shaft which Mr. Woolley ascribes to the Second Dynasty of Ur.

Among the seals of this class from other Babylonian sites there is evidence for:

(*e*) Kish 1 (*Journ. Roy. Asiat. Soc.*, 1925, 697 ff.). The description of its position is not very clear, but it seems to have lain under a pavement of Samsuiluna (*ibid.*, 1931, 593).
(*f*) Kish 2 (*ibid.*, 1931, 593): Sargonic or pre-Sargonic.
(*g*) Ashnunnak (*The Times*, 26 Mar. 1932): "private houses dating from the time of the Dynasty of Akkad."
(*h*) Telloh (*Rev. d'Assyriologie*, XXVII. 177): "niveau des objets de l'époque de Gudéa ou des restes de l'âge de Larsa."

The latest date mentioned above is the "Kassite (?) level" in (*b*), but the description shows that this object was evidently loose, and there are sufficient reasons why the Kassite period cannot be regarded as a serious possibility. The *terminus ante quem* is rather the Isin-Larsa-Babylon age (in round figures 2000 B.C.) which is indicated by (*a*) and (*e*), above. A rather earlier date is said to be possible for (*h*), while the Sargonic, or even pre-Sargonic, period is indicated by (*c*), (*d*), (*f*,) and (*g*), among which, it will be observed, are some of the most precise indications. The evidence for dating the buildings and other finds at Mohenjo-Daro is discussed in various passages of Sir John Marshall's first two volumes, with the general conclusion that, by Babylonian standards, the civilization which produced these seals flourished in the pre-Sargonic age, that is, in the earlier half of the third millennium B.C. Circumstances in India and Babylonia thus concur in revealing that there existed in the valley of the Indus a high civilization which maintained active communication with Babylonia just at a time when the civilization of that land was also standing at its height; this prosperity cannot have been fostered by anything but trade. The discovery of the Indus civilization has been an unexpected admonition of how far it may be necessary to look beyond local boundaries to comprehend the factors which governed the lives even of the most ancient peoples known to history.

It has not been thought necessary to give a copy of the inscriptions upon the seals here published since they are sufficiently clear in the photographs. It is proper, however, to point out that these inscriptions contain a number of unusual signs, judged by the material from Mohenjo-Daro. Thus, in the inscription of no. 2 the first sign (reading from right of the impression) is new, the fourth and fifth new variations. In that of no. 3 there is a long, branch-like appendage to the "man" sign and perhaps some other novelties rather obscured by the damage. There are several very strange-looking characters on no. 15, but the photograph is insufficiently clear and the original not accessible. The five signs on the large newly-acquired seal no. 18 are not indeed new, but they are written with certain peculiarities such as the disproportionate size of the "men" and the slight variant in the last two signs. Until more is known about this script it is hardly possible to suggest any reason for such peculiarities as seem to exist.

It will not, perhaps, be out of place to refer briefly to another matter which cannot indeed be elucidated, but is apposite here. In the excavations at Ur in 1926-7 two objects were found, part of a brick and a small earthenware pot, with roughly scratched inscriptions in some character that is at least not cuneiform; these are now in the British Museum, numbered 122185 and 120527 respectively. They were published by Father E. Burrows in the *Journal of the Royal Asiatic Society* for 1927, pp. 795 ff., and regarded by him as examples of a south-Semitic script. No further attention seems to have been paid to them, and I do not know whether scholars competent to judge are prepared to agree with Father Burrows's reading. Unfortunately in both cases the inscription is a mere shallow and indistinct scratch, very hard to distinguish from accidental marks on the surface, but, especially in the upper line of the brick inscription, my own examination of the original would lead to a version different in several particulars from that of Father Burrows, and containing, out of eight characters in all, four which are identical with the "Indus" signs, and three more which might less certainly be compared. In the lower line four out of five have definitely the form of signs in the "Indus" script. With respect to the pot, no. 120527, though resemblances can be found there also, the inscription is so very indistinctly scratched that there is much room for doubt. The inscription, however, is not the only marking upon the pot; under a slight ridge upon the neck is incised a plain zigzag, and just under the mouth, upon the neck, is another scratching roughly of the form \/\/\/. This may, of course, be purely ornamental, but it can hardly fail to suggest a rough way of writing the commonest sign (ᑌ) of the "Indus" script thrice over, to be compared with the pot-mark illustrated in M. pl. xc, no. 7. It should be added that the pot itself appears to be of a distinctly late type, and the brick too may be only a little earlier (*J.R.A.S.*, 1927, p. 795) than Nebuchadrezzar; this, of course, would be unfavourable to the notion of connecting them with the "Indus" civilization. It is very unsatisfactory to be obliged to leave these two interesting antiquities without a convincing explanation, and thus to have put forward a suggestion which cannot at present be substantiated, but as the matter could appropriately be mentioned here I felt bound to touch upon it.

There is one more feature in some of the seals here published which does not seem open to doubt, namely, the astral character of certain devices. This appears most plainly in No. 12, the figure of the water-carrier whose identity as Aquarius[34] is very plainly suggested by the two stars above his shoulders. It has already been noticed that the whole device on this seal is enclosed in a "parenthesis": this suggests comparison with the preceding no. 11, which almost as plainly has an astral symbol (Scorpio) accompanied by an ellipse, itself a known character of the "Indus" writing. It has recently been suggested,[35] with much appearance of reason, that the "parenthesis" is only a splitting of the ellipse when the sign to be inserted is too big to be enclosed. If this be true there is a double likeness between the seals 11 and 12; in both an astral (zodiacal) figure is associated with the ellipse. There is at least one other example of a zodiacal sign on these seals; on the recent acquisition of the British Museum, no. 18, the figures of the two men side by side (Gemini?) are marked out by their greater size among three other signs. The devices upon the cylinder-seal no. 6 are also very noteworthy in this respect: there is the humped bull, the scorpion, and above them a curiously sprawling figure of a man with a star for his head, and two small serpents. The palm-tree is not at present explicable,[36] and the "rayed" object in front of the bull is probably only another version of the "manger." The bull is so ubiquitous that it would probably be rash to regard him as astral, but if he is so then the seal no. 14 is significant, for there the bull appears in company with what seems to be the moon-god, and the comparison of this god with a bull is of course familiar in

Babylonian texts. The fish is a very common sign in the "Indus" script, but there is at present no example which gives it any such prominence as the waterman, the scorpion, and the twin have in the foregoing instances. The same is true also of the figure of an armed man (CCCLXXVIII in the Sign-Manual of M.); he is probably not an archer at all, and, even if so, there is no reason to call him Sagittarius.

If this astronomical character can hardly be mistaken in the devices of at least three of the seals here published it must be added that herein appears to lie a distinction (others have been noted above) between what may be called the "Mesopotamian" and the "native" objects of the same class. The former use, with an astronomical signification, certain figures of the script which in the latter seem to be writing-signs and nothing more. It is nevertheless as well to remember that with any "hieroglyphic" writing the distinction between a picture and a writing-sign is unreal, at least until we know how the writing is to be read, and the use of one of the signs as a figure on the shell plaque (M, pl. CXXXII, no. 10) at Mohenjo-Daro hints that a seal like the one with the waterman-figure would not necessarily have been unfamiliar on the Indus. But the use of astronomical symbols, so characteristic of Babylonia, reinforces the suggestion of the seal with a cuneiform inscription that Ur and the other cities of the land did not simply receive these objects as a strange foreign import, but took some part in the moulding of them. Herein the future has no doubt much to reveal.

NOTES

[1]The Herbert Weld (for Oxford University) and Field Museum of Natural History (Chicago) Expedition to Mesopotamia See E. Mackay in the *JRAS*, 1925:697ff.

[2]*Revue d'Assyriologie*, Vol. 22:99ff

[3]*Revue d'Assyriologie*, Vol. 22:56

[4]H. deGenouillac in *Revue d'Assyriologie*, Vol 27:177.

[5]S. Langdon in *JRAS*, 1931:593ff.

[6]H. Frankfort in *The Times*, 26 March 1932.

[7]S. Langdon in *JRAS*, 1932:47f

[8]Egyptian and Assyrian Department no. 123059:*BMQ*, Vol. 7.5.

[9]*Archiv für Orientforschung*, Vol. 4·21.

[10]*Bulletin*, 1929.28.

[11]For this discovery see Marshall (1931, Vol. II:370).

[12]Egyptian and Assyrian Department no. 120573. See also *Museum Journal*, Philadelphia, 1929: p. XLI.

[13]The boss on the back is more tubular than any shown in M. III. cii, and has no groove.

[14]The bull is the short-horned bull described in M. II. 385, and the attitude is the same.

[15]Cf. M. III, cx. 310, 312, 318, 319, 322.

[16]On the glaze see M. II. 379, and for the back M. III. cii. k.1.

[17]See M. II. 375 (three specimens only).

[18]See M. II. 371 and 381, n.3.

[19]*Antiquaries Journal*, Vol. 11:357.

[20]The humped bulls from Mohenjo daro (M. nos. 327-40) are not depicted with a "manger," and in the present example the "manger" is different from those which stand before the short-horned bulls

[21]They may be two more characters of the writing, nos. CCLXIII and CLXXIII respectively of the Sign-Manual in M. III. The former sign occurs in no. 15 and perhaps in no. 12.

[22]See M. II. 377; but in these the slope is steeper, owing to the slighter protuberance of the bosses.

[23]Compare Plate VIII, no. 14.

[24]*Antiquaries Journal*, Vol 6: pl. LVIII.

[25]See al-'Ubaid:97.

[26]See *Antiquaries Journal*, Vol. 11:pl. XLVII.

[27]*Archiv für Orientforschung*, Vol. 5.218f.

[28]See Woolley in *Antiquaries Journal*, Vol. 8, p. 3, who mentions 1 25 meters as the depth of a Sargonic grave identified by a seal.

[29]*Antiquaries Journal*, Vol. 11.362-4.

[30]It is possible that this is the seal referred to by Professor Sayce in *Antiquity*, 1927, p. 206n., as having been published by Terrien de Lacouperie many years ago in the *Proceedings of the Society of Biblical Archaeology*. I regret that I have not as yet been able to find this article.

[31]*BMQ*, Vol. 7 5ff.

[32]M. II. 371, 381, n.3, 425, n.1.

[33]At the moment of writing, and including those published here, the exact proportion is, round 17, rectangular 7, viz. round: 15 in the present article, Delaporte, *Catalogue des cylindres du Louvre*, pl. II. 8; *JRAS*, 1932, 48, rectangular: *JRAS*, 1925, 697, and 1931, 595; *RA*, Vol. 27·56 and 99, *Archiv für Orientforschung*, Vol. 4:21; "cuneiform" seal from Ur (no. 1 here), square stamp from Ashnunnak (*The Times*, 26 March 1932). The shape of the one reported by de Genouillac in *Ra*, Vol. 27.177 is not stated.

[34]It may be observed that the "waterman," as such, was unknown to the Babylonians both in name and figure The corresponding stars were probably those called ^{mul}gu-la, i.e "the great constellation," and there is no representation of an astral waterman known in Babylonian art. If the figure on this seal is rightly interpreted it would, of course, be the earliest by far of all appearances of this mythical personality.

[35]By Mr. G.R. Hunter in *JRAS*, 1932:476.

[36]Though it should be mentioned that the palm-tree appears in company with astral symbols on the Black Stone of Esarhaddon in the British Museum.

Further Links between Ancient Sind, Sumer and Elsewhere

E.J.H. MACKAY

During my leave to England in the spring of 1930, I revisited Iraq to acquaint myself further with the objects on view in the Baghdad Museum. As a result of excavations since I left that country the museum had acquired a large increase of material that I was not familiar with before, and I am now able to add to the links between the cultures of the Indus Valley and of Sumer which I have already pointed out in *Mohenjo-daro and the Indus Civilization* (Marshall 1931).

On Plate 146, Fig. 43, of that book there is reproduced a red carnelian bead, bearing a somewhat elaborate design in white, that was found in the uppermost levels of the VS area. Several almost exactly similar beads found by Woolley in the early graves at Ur are now in the Baghdad Museum. The slight difference between the two designs, in that there are concentric circles on this first found Indian bead of the type in place of the single circles on the specimens from Ur, is negligible in face of the general similarity; and especially in view of the fact that another of these beads has since been discovered at Mohenjo-daro with single circles, so that the design is identical with that on the beads from Ur (Plate IX, no. 2).

It should be noted that on none of these beads do the oblique lines join the corners, an arrangement which, though somewhat unexpected, was probably regarded for some reason as essential to the design.

From the somewhat enlarged illustration of the second Indus Valley specimen, it will be seen that the craftsman was not entirely familiar with the design; its lines show evidence of hesitation. It is conceivable, indeed, that this particular specimen is a local copy of an imported bead.

The beads from Mohenjo-daro appear to be slightly squarer than the Sumerian specimens, but it is possible that my hurried sketch of the latter has erred in this respect. The shapes of these beads are, of course, inconclusive as evidence of a connexion between the two countries, but the quite definite resemblance in design, especially in conjunction with the difficult technique, proves beyond any doubt that the beads found at Mohenjo-daro and Ur were made in one and the same country—whether this was India or Sumer we do not yet know; the beads may conceivably have come from a third country in the course of trade.

Woolley found the beads of this type in the Royal graves of Ur. Personally, I would like to date these graves later for reasons to be stated at the end of this note. But in any case, we have in these beads proof that the civilization of Mohenjo-daro can hardly be dated later than 2750 B.C., and it is quite possible, if Woolley's chronology be correct, that even the latest levels of Mohenjo-daro date from before that time,[1] for, it should be repeated, the beads in question came from the uppermost strata.

In connexion with these most interesting decorated carnelian beads, Mr. N.G. Majumdar of the Archaeological Survey of India has drawn my attention to a decorated carnelian bead found by him at a site called Chanhū-daro, Nawab Shah district, Sind, the lowest levels of which belong to the Indus Valley culture. It is a flat oval bead with a figure-of-eight design, and he points out that it is exactly similar in shape, material and mode of decoration to a bead that I found at Kish and illustrated in my second report on that site (Mackay, 1929a: pl. 60, f. 55). There are yet other examples of these decorated carnelian beads which serve to link the two civilizations; one (Marshall 1931: pl. 146, f. 62) exactly resembles, both in shape

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and in its marginal decoration, a bead found at Kish (Mackay 1929a: pl. 60, f. 62).

Another design in carnelian unearthed at Kish (Mackay 1929a: pl. 60, f. 54) also occurs, I believe, at Ur. It is singularly like a design that is exceedingly common at Mohenjo-daro, especially as shell inlay and on nose studs (Marshall 1931: pl. 152, f. 14: 155, f. 48, 49: 158, f. 10). There is, however, the great difference between the Sumerian and Indus Valley examples that in the former there are five loops, whereas there are four in the latter. Judging from its popularity at Mohenjo-daro, the design must have been regarded with especial favour; and I have a suspicion that the bead from Kish was an inaccurate copy of those of Mohenjo-daro, although it should be remembered that the number five seems to have had an especial significance in Sumer. In either case, the Kish specimen could hardly have been made in India.

It would certainly be of great interest if we could discover whence these carnelian beads were imported. As I have already stated, they are extremely rare at Mohenjo-daro, and that they were highly valued is proved by their being imitated in steatite, on which the red ground for the design was produced by means of a burnished haematite paint. Perhaps Persia, where I believe the painting of carnelian is still carried on, was the source of supply both for Sumer and Sind; if so, that country could also have supplied Russia where, according to Mr. Beck, decorated carnelian beads have been found, although some are of comparatively late date (Thompson and Hutchenson 1929:144).

The rectangular bead illustrated here (Plate IX, no. 7) comes from the upper levels of Mohenjo-daro. It measures 1.6 ins. long by 0.26 ins. thick, and in shape it is very like a bead found in the grave of Queen Shubad at Ur (Legrain 1929: pl. V). I believe I am right in stating that these beads are both agate, though the one from Mohenjo-daro is not quite so translucent as the example from Ur. Beads of this shape are so very rare at Mohenjo-daro that they may have been imported.

Another bead shown (Plate IX, no. 3) still more closely resembles beads that have been found at both Kish and Ur. Its shape is very peculiar in that the sharp edges of the longer sides are notched. This particular bead is made of some kind of paste which was at one time glazed; it is rhomboidal in section and measures 0.5 in. long by 0.1 in. thick. There are specimens of this type of bead from Ur in the Baghdad Museum, and two examples are figured in the Kish II report (Mackay 1929a: pl. 60, f. 39-40 and p. 186). The latter are made of shell and were found in the A graves. The three specimens that have been found at Mohenjo-daro are made of either alabaster or faience. The scarcity of these beads at Mohenjo-daro as compared with their relative plentifulness in Sumer suggests that they came to India from the latter country. If this be so, the curious shape and the comparatively inexpensive materials in which they were made suggest that they had an amuletic rather than an aesthetic value. They are very similar in shape to the reversible sickle-flints (Plate IX, no. 4) that are so often found on ancient Sumerian sites, and it is possible that the beads are actual copies of the flints. If so, it is not at all improbable that the beads were worn as fertility charms, since the sickle-flints were so closely associated with agriculture.

The terminals of flattened hemispherical shape (Marshall 1931: pl. 149, f. 1-3, pl. 151, f. b) which are so frequently found at this site are made, as has already been stated, in gold and copper, and, more rarely, in faience. In the latter material they are either solid, except for the holes to take the strings, or else their sides are very thick, a necessary precaution with such a brittle material. I find no mention of terminals of this shape ever being found at early Mesopotamian sites. I myself found none at Kish, nor have any from Ur been illustrated. That they will eventually be found in that country, may, however, be considered as probable, especially as they have been found at Byblos, where they date from the period of the Fourth Dynasty of Egypt. These Syrian examples, which are of faience, are solid and have several holes pierced through the straight edge instead of through the terminal to its apex; i.e., the ends of the strings of beads were tied to, and not passed through the terminals. Whether these terminals are Egyptian or Indian in origin or invented independently, is uncertain; hollow gold terminals which exactly resemble the examples from Mohenjo-daro have also lately been found at Gizeh in Egypt by Prof. Selim Hassan, who dates the tomb in which they were found to the Fourth Dynasty. The latter include—as far as I can judge from the published photographs—a long narrow gold plate pierced with holes to seal up the open end of the terminal, as at Mohenjo-daro.

Of still earlier date are some representations of hemispherical and triangular-shaped terminals with four or five undoubted strings of beads attached (Fig. 13.1). These are depicted on four painted pottery sherds found at Tépé Douecya, some three kilometres

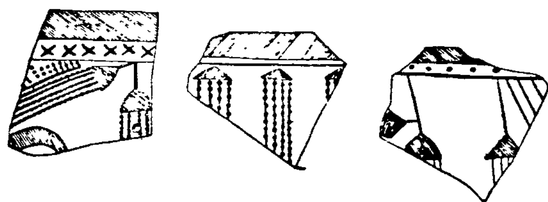


FIGURE 13.1. *Fragments of painted vases, Tepe Doueeya.*

north of Susa, and dated by M. de Mecquenem to the time of the First Period of the last site.

I have already compared terminals of this shape with some belonging to the Eighteenth Dynasty, but it is of interest to see that in Egypt the type goes back to much earlier days.

A copper blade (Marshall 1931: pl. 136, f. 3) found in one of the upper levels, though termed a spear-blade, may conceivably have been a knife (Plate IX, no. 1). An exactly similar blade, but with a slightly longer tang, was found in the A mound at Kish (Mackay 1929a: pl. 39, gp. 3, f. 4).

Until five years ago it seemed that cubical dice were unknown in the early civilizations, but we now have many examples from Mohenjo-daro (Marshall 1931: pl. 153, f. 7-10), and one has been found at Ur, where it has been dated to an early period. The arrangement of the numbers in the specimen from Ur differs slightly from the arrangement on the dice from Mohenjo-daro, and a rosette even takes the place of one of the numbers. The only really ancient cubical die that I know of west of Suez is a pottery one with painted points, found at Tel el Amarna by the Egypt Exploration Society in 1921. Curiously enough, the numbers are apparently arranged in exactly the same way as at Mohenjo-daro, that is to say, 1 is opposite 2, 3 opposite 4, and probably 5 opposite 6. Sir Flinders Petrie has illustrated a number of cubical dice which he has found or bought in Egypt (Petrie 1927: pl. 49), but they are all of Ptolemaic or Roman date. Some are numbered as at Mohenjo-daro, some in the modern way, and others quite indifferently. The fact that the cubical die occurs as far back as the Eighteenth Dynasty in Egypt is important, as it disposes of the suggestion that the Greeks were the means of introducing it into the West.

I took the opportunity when at Baghdad to examine the tetrahedron of pink limestone found by Woolley at Tell al'Ubaid and described by him in the report on that site (Hall and Woolley 1927:211, T.O. 403), and I find that in shape it is exactly similar to the many that have now been found at Mohenjo-daro (Marshall 1931: pl. 153, f. 40-1). Of these only one specimen, however, is made of pink limestone; the remainder were made of a paste which was coated with a glaze, once either green or blue in colour. Gamesmen of the same form and material have also been unearthed by Watelin at Kish. Whether the peculiar shape of these tetrahedral gamesmen originated in Sumer or India, it is impossible to say. I am not aware of their occurring outside these two countries.

When at Kish, I found a number of thick pottery rings, whose internal diameters average 1 in. As they are much too small to have been worn as bracelets, and for the same reason would have been unsatisfactory as stands for pointed and round-based pottery, it seems likely that they were used in a game—like quoits (Mackay 1929a: pl. 44, f. 2, p. 206). Rings of the same type and material are frequently found at Mohenjo-daro (Marshall 1931: pl. 152, f. 16 top), and it is possible that here also they were used as playthings; experiment has shown them to be practically useless as pottery stands.

It has already been pointed out that a framed Greek cross was used as a decorative motif at Mohenjo-daro, and that the same device was also well known in ancient Sumer, as well as being used as an ornament on seals of the Kassite period. But its repetition as a decorative design, as, for instance, on the square bezel of the silver ring (Marshall 1931: pl. 148a, f. 13) is unusual. Nevertheless, it has been found duplicated on one of the squares of shell of a gaming board (no. U 9907) discovered by Woolley at Ur. Additional interest is given to the design in this latter case by the presence of numerous svastikas that fill in the spaces between the crosses. Though these are not present in the design on the silver ring (Plate IX, no. 5) and squares take their place, there is the possibility that the svastika motif originated in India, even though it appears also on the early painted pottery of Elam (deMorgan 1925/27:266, Fig. 293) as well as that of Sumer (Herzfeld 1927: V, 16, 17). Both the appearance of the Greek cross motif and its arrangement on the objects cited from Sind and Sumer add an undoubted link between the two countries, and a very early one at that.

With reference to the svastika, attention should be called to a steatite seal from Kish, now in the Baghdad Museum, which bears this symbol. This seal, both in shape and the design upon it, exactly resembles the little square seals of steatite and glazed paste that are so frequently found at Mohenjo-daro (Marshall 1931:

pl. 144, f. 507-15). I do not think that I err in regarding the Kish example, which was found by Watelin, as either of Indian workmanship or made locally for an Indian resident in Sumer.

Feeding cups with a spout projecting upwards from the base (Marshall 1931: pl. 83, f. 20) are well known at Mohenjo-daro. It was, therefore, pleasing to find that the magnificent gold specimen found at Ur is not an isolated example, but that this type of cup was also made there in pottery—again, at a very early period.

We have already shown that the working of shell was common to early India and ancient Sumer, this material being used in both these countries chiefly for inlay. A form of ladle made in shell, which is frequently found at Mohenjo-daro (Marshall 1931: pl. 156, f. 26-9), is exactly duplicated in examples found at both Kish and Ur.

Again, the capping of finely cut, variegated hard-stone beads with gold (Marshall 1931 :pl. 149) was also practised in Sumer, for Woolley has found such beads in early graves at Ur. Indeed, the close resemblance of some of these capped beads to those of Mohenjo-daro leads one to suspect that they were actually of Indian workmanship. I base this suggestion on the fact that they are more common at Mohenjo-daro than in Sumer.

The curious perforated vessels shown (Marshall 1931: pl. 84, f. 3-18) are very closely allied to perforated vessels found at Kish (Mackay 1929a: pl. 54, f. 36), especially in the fact that besides the numerous holes in the sides there is also a large hole in the base, which suggests that by this means they were supported on a rod or something similar. The shapes are certainly different, but this fact is of little consequence in view of the general difference in the shapes of the pottery used by the Indus Valley peoples from the wares found in the early cemeteries at Ur and in the A graves at Kish. I have suggested, from evidence obtained by Sir Aurel Stein in southern Baluchistan, that these perforated vessels were used as heaters. But I am now inclined to believe that they served as strainers for curds. We know from the presence of numerous models of oxen in their cities that cattle-raising was practised by the Indus Valley people; milk was, therefore, in all probability as much used by them as by the Sumerians throughout their history.

The carefully modelled panther heads made in pottery (Marshall 1931: pl. 96, f. 5-6) are obviously intended to be affixed to a backing of some kind, since they are both hollow behind. Their striking resemblance to similar masks found at Ur, though the latter are of silver, provides a further link between the cultures of Sumer and ancient Sind. The silver heads from Ur (Woolley 1928a :16) were once affixed to the front of a royal chariot. We have no reason to suppose that the Mohenjo-daro heads were used for the same purpose; indeed, the humbler material of which they are made suggests that they beautified something much less important, and we hope to discover later on what that was. I have already compared the other type of mask found at Mohenjo-daro, i.e., a human head with the horns of a bull, to the similar metal heads from Ur, a parallel which certainly suggests that a deity of the same form, if not with the same attributes, was common to the two cultures.

The little pottery figures of doves with outstretched wings, that are so frequently found at Mohenjo-daro (Marshall 1931: pl. 96, f. 1) are, as I have already explained, known in early Crete as well as at Musyan in Elam. It is, therefore, interesting to see that Woolley has found a similar figure at Ur beneath a deposit left by a flood. The only difference between the Indus Valley specimens and the one from Ur, beyond that of the kind of clay employed, is that the latter is painted. It has a hole in its base, doubtless that it might be supported on a stick; and we have a similar hole in many of the examples at Mohenjo-daro, though it is lacking in the particular specimen illustrated. Another form of the same bird, with closed wings, and set on a little pedestal, is common both at Kish (Mackay 1929a: pl. 47, f. 10) and Mohenjo-daro (Marshall 1931: pl. 153, f. 17-8). In the latter city a small hole in the back near the tail sometimes turned the model into a whistle, but model birds of the same shape are found without this added device.

In ancient Sumer the dove seems to have been included in offerings to the various deities, and Eannatum states that he offered two to the goddess Ninkharsag (King 1916:128-9). At a yet earlier date figures of the bird occur in the limestone inlay which once decorated the temple of al'Ubaid, that was also dedicated to the goddess Ninkharsag (Hall and Woolley 1927: pl. 33); and that the bird was sacred in Elam seems certain from the lapis-lazuli and gold figure found at Susa by the French expedition.[2]

The close association of the dove with the cult of the Mother-goddess in Crete, Sumer, and elsewhere in the Near and Middle East, in Sardinia, and even further west, together with the fact that so many models of this bird are found at Mohenjo-daro, leads us further to

believe that the goddess whose semi-nude, bejewelled pottery images are such a feature of Mohenjo-daro and Harappa was also a Mother-goddess. The great respect in which the dove is held even at the present day in Northern India by Muhammedans and Hindus alike is quite possibly a survival of this cult (Cooke 1926:246). Perhaps there was a closer connexion than we at present know of between the Sumerian goddess Ninkharsag and the goddess of the Indus Valley people.

The fine steatite head (Marshall 1931: pl. 98, f. 1-4) has peculiar, half-closed eyes, which according to Mr Ramprasad Chanda proves the practice of "Yoga" among the people of the Indus Valley (Chanda 1929). But the curious figurines found by Woolley in graves of the al'Ubaid II period at Ur are also represented with this very narrow eye (Woolley 1930: pl. 48, a-d; Hall and Woolley 1927: pl. 48). Woolley describes these heads as reptilian, but, personally, I believe that they are intended to represent human heads, a view which is somewhat corroborated by a human figure holding a bow and arrow which Dr Herzfeld found on a painted sherd at Susa, for the shapes of the head and head-dress of this latter figure are identical with those of the Ur figurines.[3] Possibly these Sumerian figures were intended to represent the autochthonous inhabitants of the country.

We have yet a fourth example of this very unusual eye in a pottery head that I found some time ago at Kish, and that I believe is now in the Ashmolean Museum at Oxford (Plate IX, nos. 8-9). Unfortunately, this curious head cannot be accurately dated, but that it is archaic admits of no doubt. The fan-like head-dress or arrangement of the hair is also very unusual and has something in common with the head-dress worn by some of the female figurines of Mohenjo-daro.

I cannot but think that the unusual narrowing of the eye in all these very early examples suggests its association with some definite idea held in common, but whether religious or racial it is impossible to say. The human figures of about this time portrayed on jar handles at Kish, though all female, are, it is true, without exception represented with round, open eyes, such as are generally associated with Sumerian sculpture (Mackay 1929a: pl. 45). The absence of the mouth in the above mentioned pottery head from Kish is not surprising in view of the fact that this feature is also frequently absent in the figures on the jar-handles of the A cemetery. The figures of the inlaid plaque from the palace at Kish have unusually small mouths, and so has the large painted terra-cotta head since found there by Watelin (1929). Indeed, the mouth seems to have been treated as a somewhat unimportant feature in some of the early representations of the human head in Sumer. I would like to point out in addition that, so far as it is possible to judge from a photograph, the ears of this last mentioned head are exactly the same roughly modelled, saucer-like features as the ears of the statue heads found at Mohenjo-daro.

Though not strictly germane to this article, it is necessary to mention that the handled vessels from Kish had a wider range than was at first realized. One such vessel has been found at Ur, and a handle at Susa, the latter dated to about 3000 B.C. Apparently Contenau, who mentions it, has not realized that this is a handle of a jar of the Kish type.[4] These curious vessels may have been taken to both Ur and Susa as they seem not to have been made in those places.

The stone jar-borer (Marshall 1931: pl. 130, f. 35) has already been compared with one from Egypt, but of much later date. Woolley has also found one at a site named Meraijib, 11 miles south of Ur, which is a very early example indeed,[5] and of very much the same in shape, as I have seen for myself. Though not of great importance as evidence in the dating of Mohenjo-daro, for the reason that the identical shape is known at a later period in Egypt, this borer proves that the same class of implement was used in the two countries. In all probability these stone grinders were heavily weighted and rotated by means of a crank; the same method was practised by the ancient Egyptians, who on their tomb walls have given us many illustrations of how they were used (Murray 1922:18, Fig. 40; Clarke and Engelbach 1930:204). Woolley suggests that these borers were worked by the aid of a bow and cord. I think that the friction that would be created by the rotation of such a heavy object would be too much for the bow and cord method.

The elaborate figure-of-eight design seen on a copper tablet (Marshall 1931: pl. 118, f. 5) cannot be exactly duplicated in Sumer. The simple outline was, however, frequently used at Mohenjo-daro to ornament beads, and it also appears, as has already been stated in this note, on a painted carnelian bead from Kish. Of great importance is the fact that identically the same design as that incised on the copper tablet appears on two scarabs approximately dated by Petrie to the Thirteenth to Seventeenth Dynasties of Egypt, where one would hardly have expected it (Petrie 1925: pl. 8, Figs. 129-30). It is also painted on a stone flake,

dated to the Eighteenth Dynasty found at Tell el Amarna in 1922 by the Egypt Exploration Society and is now in the Ashmolean Museum, Oxford.

I have no doubt that this same cord pattern will eventually come to light in both Elam and Sumer. That it took so long to travel to Egypt from its occurrence in India in the earlier part of the third millennium B.C. until the second millennium B.C., is a point of interest; but in time this period may be considerably reduced, for something very like this design, though more complex, occurs on a predynastic vase from Egypt, as I have already noted. This particular predynastic vase is closely dated by both shape and style of painting to the second, or middle predynastic period, as Hornblower has already pointed out (Hornblower 1928:68-9), and Petrie has more than once suggested that the home of the people of this sub-period was "somewhere bordering on the Red Sea," or "possibly southern Sinai or the northern Hedjaz" (Petrie 1920:48). In short, he regards the people of the second predynastic period as of Asiatic origin. Indeed, there is increasing evidence that certain ideas from Middle Asia did actually filter slowly to Egypt, probably in most cases via Syria; but whether these ideas were Indian in origin or Mesopotamian, with which we must couple Elam, we do not know.

The clay animal on wheels, which Woolley found at a very early level at Ur and described as a zoo-morphic vase (Woolley 1930: pl. 41a), has its almost exact counterpart in a broken toy that I found in the debris covering the Sumerian palace at Kish (Mackay 1929a: pl. 46, f. 3) (Plate IX, no. 6). I have described the Kish figure as a ram, to which opinion I still adhere, and the Ur figure seems to me to represent the same animal. The Kish specimen is certainly not a vase and was only made hollow for the sake of lightness, which is almost a necessity in a toy made for a small child. Woolley dates his example to the period of the royal cemetery, or perhaps even earlier; but the Kish specimen must have been considerably later.

These toy animals from Sumer are closely comparable with the toy rams, also on wheels, that we occasionally find at Mohenjo-daro (Marshall 1931: pl. 153, f. 24). The fact that two wheels were preferred to four in ancient Sind matters little; we have the same animal represented and also with a hollow body for the sake of lightness. Imagination, however, was given wider range in the Sind examples in that a bird's tail is introduced which brings them into the category of "composite" animals, of which the people of Mohenjo-daro, and be it noted, of Sumer also, were so fond.

The decorative design of four-petalled rosettes that is such a common feature on the painted pottery of Mohenjo-daro (Marshall 1931: pl. 91, f. 9, 10) is also met with on painted pottery from Tell Zeidan on the eastern bank of the lower Balikh river in North Syria. As we know, pottery designs were adopted by neighbouring peoples, but it is interesting to find a motif that was in such common use in the Indus Valley occurring as far away as northern Syria, and not in the countries between. Such a simple motif may, however, have been designed independently.

In connexion with the two pieces of glazed pottery (Marshall 1931: pl. 159, f. 1, 2), the method of whose ornamentation has already been described, I should like to point out that the same device of partially removing a dark-coloured slip with the aid of a comb or other such instrument is also known at Ur. This "reserved slip ware," as it is termed, occurs in Woolley's stratum E, a very early level (Woolley 1930:331 and 339), and does not differ in general technique from the similar ware at Mohenjo-daro, though the latter was glazed. We have, however, found one example where a light slip has been removed in parts from the surface of unglazed pottery instead of, as at Kish, a dark slip from a lighter ware. It is of interest to note that this "reserved slip" ware, whether glazed or not, is only found in the lower levels of Mohenjo-daro, which appear to correspond with those of Ur.

I have already remarked that as far as can be seen, no strict comparison can be made between the shapes of the pottery from Mohenjo-daro and the early wares of Sumer and Elam. There is, however, one exception. We find, principally in the upper levels of Mohenjo-daro, though it is also known in the intermediate phases, a type of vessel fashioned sometimes in copper and bronze, but more generally made in clay (Marshall 1931: pl. 86, f. 1, 22; pl. 140, f. 18). An alabaster vessel in the Baghdad Museum, which is labelled as having come from Khaffaga near the Diala river, corresponds very closely in shape with this Indian type. Though I do not know its exact date, it appears to me to be archaic.[6]

These additional facts, together with the connexions already pointed out in the book, prove beyond question that the upper occupations at Mohenjo-daro are contemporary with the earlier ones of Ur and Kish. I connect these two latter sites because I am convinced that the graves of Woolley's third series, the "gold

graves," as they are often termed, as well as his second series, belong to very much the same period as the A graves at Kish, though he appears to regard the latter as contemporaneous only with his second series. There is, I consider, too great a similarity between the objects found in the A cemetery and the third series cf graves at Ur for any great space of time to have intervened between them. It is true that many articles found in these latter graves have not been found at Kish, but their absence is easily explained by the fact that the people buried in the "gold graves" at Ur were immeasurably richer than those at Kish, where the graves were obviously those of comparatively poor people.

We have assigned a somewhat conservative date to the upper levels of Mohenjo-daro, but we may have to increase it in view of the many connexions between that place and the early periods of Kish and Ur. In the present state of our knowledge, the connexions that have now been established between the two countries do not allow of any other course. On the other hand, when the chronology of the Sumerian graves is finally settled, their dates may be brought nearer to our estimate of the date of the late levels of the Indus civilization.

Since the above lines were written, Watelin has kindly sent me a photograph of a seal of the Indus Valley type that he has lately found at Kish. There is no doubt whatever that this particular seal was made and engraved in India. The first seal of this type to be found at Kish came from a level dated approximately to 2100 B.C., but as it was found in the filling of foundations of this date, there was reason to think that the seal itself was of earlier date and had lain for some time in the earth that was used for the filling. That it was already old when it found its last resting place at Kish is proved by its broken and badly weathered condition (Mackay 1925:697). As the characters and animals on these Indian seals must have been more or less strange to most of the Sumerians, it is not unlikely that whenever they were picked up at a later date they were kept as curios.[7]

NOTES

[1]Editor's note: The chronology for Mohenjo daro is considerably changed from Mr. Mackay's estimates published in this paper Many of his dates have been edited from the original version as published in *Antiquity*.

[2]*Memoires du la Delegation Francais en Perse*, Vol. 7, pl. 25, no 1, 2.

[3]*Memoires du la Delagation Francais en Perse*, Vol. 13, pl. 37, no 129.

[4]Contenau (1927/47:178, no. 107). A complete specimen of one of these "granny" jars has lately been found at Susa (deMecquenem 1931 : pl. IX).

[5]It would seem to be of the Jemdet Nasr period.

[6]I am given to understand that it was bought by the Museum.

[7]For a parallel, note the antiquarian tastes of Nabonidus and his daughter (Woolley 1925:384).

Trade Mechanisms in Indus-Mesopotamian Interrelations

C.C. LAMBERG-KARLOVSKY

Trade may be understood in its widest sense as the reciprocal traffic of materials or goods directed by human agency from one place and/or individual to another. Polyani (1957:159) divides the mechanics of trade into four major constituents which provide a suitable framework within which to examine trade: two-sidedness, goods, personnel and carrying. Our emphasis will be upon the first three. Our information on the last for the time period involved, save for the presence of sea-faring, is virtually *nil.* Additionally at least three different processes in long distance trade can be profitably distinguished.

1. *Direct Contact Trade.* Face to face contact is established between two different places for the purposes of trade. Goods are traded between places A and B without direct assistance by or relations with intermediary sites. This may include the actual presence of trading colonies established by peoples of place A at site B for the trade of specific materials of standardized value. This type of trade is usually centrally organized and administered by one of the principals involved (see Fig. 14.1).

2. *Exchange.* This form in the dissemination of goods differs from the above by lacking a definite organization or standardized value of specific materials. Goods are passed from place to place without specific design or purpose. Thus materials from site A and their arrival at the site B represent an arbitrary exchange of merchandise from site to site. It is often difficult to isolate whether an object was brought into a site through exchange or independently produced through stimulus diffusion of a style or functional tool type.

3. *Central Place Trade* is evident when goods are either produced, or resources present, at a few necessarily central points. Thus site C may be located beyond the spheres of influence of sites A and B and control the means of production and/or resources which are desired by sites A and B. Site C, acting as a Central Place, may then either transship materials produced in other centers or export its own materials or resources. Alternatively, the resources and/or transshipment of goods may be under the control and direction of peoples from either site A or B residing among the foreigners of site C. In this respect there is Direct Contact Trade between the Central Place (site C) and either A and/or B. The important factor is that the Central Place (C) is of a different culture than either A or B. It becomes immediately apparent that the archaeologist must attempt to distinguish whether peoples from sites A or B are physically present, i.e., in the form of a trading colony at the Central Place or whether material remains of A or B are present at C as a result of trade.

Insufficient emphasis has been placed on the economic development of trade in what may have been independent systems or mechanisms. Three such systems are described as above and diagrammed in Fig. 14.1. They appear most profitable as isolated mechanisms in discussing Indus-Mesopotamian relations. It must be recognized, however, that these are not mutually exclusive systems—all three types may be coexistent. The task of the archaeologist is to distinguish which process is involved in any trade mechanism at a given point in time. Because of the high cost of transportation, long distance trade is mainly restricted to materials and goods which are of great value or produced and/or available in limited areas. The role that trade may have had in generative processes leading *toward* urbanization is unknown. It is unreasonable to dismiss long distance trade on *a*

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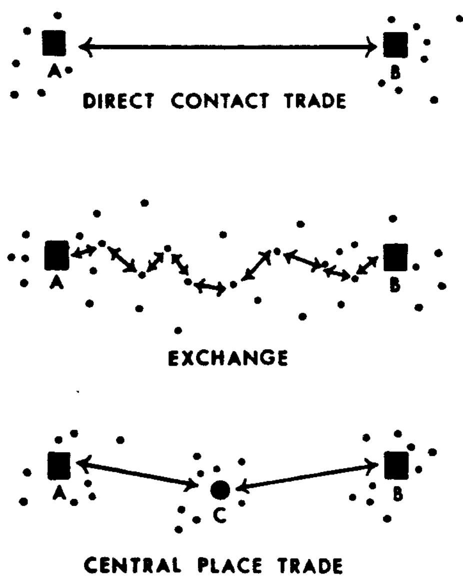


FIGURE 14.1. *Three processes in long distance trade.*

priori grounds as derivative from the growth of urban civilization rather than having perhaps helped bring the latter into existence.

Until recently archaeologists have argued for the predominance of *Direct Contact Trade* between the Indus and Mesopotamia, either by sea (Oppenheim 1954) or by land (Mallowan 1965).

DIRECT CONTACT TRADE

Direct contact between traders or colonies within the Indus and Mesopotamia cannot be supported or negated by the archaeological evidence. Clearly a handful of seals (Gadd 1932; Wheeler 1968), etched carnelian beads (Wheeler 1968; M.G. Dikshit 1949), terracotta statues and dice (Dales 1968c) in Mesopotamia cannot be used as concrete evidence to support the presence of Indus traders in Mesopotamia. Conversely, the presence of perforated, knobbed and "reserve slip" ware (Delougaz 1952), spiral and animal headed pins (Piggott 1948) or segmented beads (Wheeler 1968) cannot support the evidence of Mesopotamia in the Indus.

Direct Contact Trade through the presence of Assyrian trading colonies at the Anatolian sites of Hattush (Bittel 1970) and Kultepe (Ozguc 1962) have been concrete examples derived from excavations of this type of trade. At both sites the excavators argued for distinctive activity areas inhabited by foreign trading colonists, set apart from the living quarters of the indigenous inhabitants. It is instructive to see the evidence which suggested this situation to the excavators. The intrusive nature of the colonists was not obtained by identifying the distinctive nature of their architecture or ceramics, which were similar to those of the indigenous inhabitants. At both the karum of Kultepe and Hattush the presence of the colonies were indicated by the presence of cylinder seal impressions and textual data. Thus at Kultepe the excavator admits, "If the tablets and their sealed envelopes had not been found, in fact, we might never have suspected the existence of a merchant colony" (Ozguc 1962). At Hattush of sixty-three Old Assyrian documents, sixty were found in the residential quarters of the colony and "These documents contain only Assyrian names, no native ones. The impression of cylinder seals on the envelopes show, without exception, Mesopotamian non-Anatolian motifs."

On analogy, the principal evidence for Direct Contact Trade between Mesopotamia and the Indus should be seen in the seals, sealing impressions and textual data of one culture found in another. No surprise, for, if Indus traders were in Mesopotamia, or the reverse one would expect them to seal goods shipped back by their own seals. Some points become immediately evident:

(1) No Mesopotamian seals, sealings or texts have ever been found in Harappan context.

(2) Only one Indus type seal impression with ten Indus signs has been found in Mesopotamia (at Umma, Scheil 1925).

(3) No distinctive architectural complex of Mesopotamian characteristics has ever been excavated in the Indus. The reverse being also true.

(4) On no Mesopotamian site is there a clustering of Indus objects in association with architecture, save for Tell Asmar (see below). We are left only to consider the scattered Indus-type seals in Mesopotamia—by itself weak evidence to suggest Direct Contact Trade.

Indus seals have supported the main evidence for Direct Contact Trade between the Indus and Mesopotamia, as well as for establishing contemporaneity of both civilizations during pre and post Sargonid times. It is unfortunate that the chronology is rendered doubtful by either unstratified or uncertain context of the majority of the seals.

A re-assessment of the context of the Indus seals in Mesopotamia in no way supports their association within a trading colony context. For the most part the seals are single finds without clusterings of additional

Indus type materials in association and, we might add, in an almost universally bad context.

The list below does not include the Persian Gulf types included by Gadd (1932).

Indus Type Seals

Ur

(1) Unstratified (Woolley 1928:26, Pl. XI, Fig. 2; Gadd 1932, No. 1).

(2) From Bur-Sins Tomb or mixed in later filling (Woolley 1932:362-64, Pl. LXII, No. 2; Gadd 1932, No. 16).

(3) From a vaulted tomb of Larsa Period (Gadd 1932, No. 6; Legrain 1951:632).

The remainder of the Ur seals published by Gadd (1932) have been seen as Persian Gulf variants (Wheeler 1968).

Tell Asmar

(1) From an Akkadian house, a cylinder seal (only six cylinder seals have been found in Harappan context) depicting elephant, rhinoceros, crocodile (Frankfort 1933:51; 1938:305).

(2) Akkadian context but without further association (Frankfort 1933:52).

Kish

(1) Square steatite seal with "unicorn" and the Indus signs. Found "nine meters below the surface" (Langdon 1931b:593-96).

(2) Square Indus seal with unicorn and Indus inscription, "below the pavement of Samsuiluna, son of Hammurabi" (Langdon 1931b:593).

Umma (Tell Jokha)

(1) An impressed square clay sealing with at least ten Indus signs (Scheil 1925).

The seals thus present not only doubtful chronological markers but minimal support for the presence of Indus trade in Mesopotamia. Only the Umma sealing indicates a receipt of goods received from the Indus. It is indicative, however, that such evidence as does exist, supports the presence of Indus traders in Mesopotamia rather than the reverse. Thus, if there were Direct Contact Trade it would seem to be Indus traders in Mesopotamia.

A review of the contextual association of unmistakeably Indus seals in Mesopotamia does not support the clustering of such material on any given site, nor more particularly, without a specific activity or habitation area. A single exception could be Tell Asmar where the seals are reinforced by ceramics; knobbed ware, etched beads and kidney shaped inlay of bone, all Harappan types and found in Akkadian houses at Tell Asmar. If anywhere in Mesopotamia we have evidence for Indus Direct Contact Trade it would be best supported at Tell Asmar.

Furthermore, when the material evidence for Direct Contact Trade between the Indus and Mesopotamia is compared quantitatively against the Direct Contact Trade as it existed at the Assyrian karum of Hattush and Kultepe, Egyptian-Minoan or Egyptian-Syro-Palestinian (i.e., Byblos) it becomes evident that neither a single Mesopotamian nor Indus site indicates a comparable clustering of materials in association to suggest Direct Contact Trade between these two areas.

One cannot, however, deny the existence of materials other than Indus seals in Mesopotamia which suggest some form of relations. These objects, few in number and varied in type, cannot be argued as standardized or valued trade objects (see below). Such objects may be taken as either possessions of Indus traders in Mesopotamia or having arrived through an indirect contact trade, i.e., Exchange. These materials: terracotta statues, dice, etched carnelian beads, stone vessels (see below) are not found on any single Mesopotamian site in sufficient numbers and in clustering association to support the presence of trade in these items or the presence of an Indus colony. Again, we note that incontestable Mesopotamian products simply have not been found in the Indus (see below).

The presence of single objects of Indus derivation found on Mesopotamian sites may well have been brought to Mesopotamia by hand-to-hand (site-to-site) exchange. It is unfortunate that few sites of Eastern Iran and Baluchistan between the Indus and Mesopotamia have been excavated, and those that have clearly support their role in *Central Place Trade* (see below) controlling either the given resources of an area or the transshipment of goods from the East to the West, or the reverse.

Exchange (Indirect Contact Trade)

The evidence for Exchange, an informal non-centrally administered stimulus diffusion of materials, can be supported in the distribution of materials appearing as rare occurrences both in the Indus, Mesopotamia and on sites between both areas. They are not objects or

materials upon which a reciprocal trade would be structured, i.e., necessary resources or desirable luxury goods. They are single and varied objects, as animal headed pins, beads, etc. not classes of distinctive functional materials as seals. Such an Exchange, unlike Direct Contact Trade, would not be under administrative control, but the varied materials in passing from hand-to-hand (site-to-site) would appear randomly on sites between the Indus and Mesopotamia as well as on Mesopotamian and Indus sites.

One of the best explications of an exchange system in antiquity is contained in Herodotus (*The Histories*, Book IV, Chap. 33):

> But the persons who have the most to say on this subject are the Delians. They declare that certain offerings, packed in wheaten straw, were brought from the country of the Hyperboreans into Scythia, and that the Scythians received them and passed them on to their neighbors upon the West, who continued to pass them on, until at last they reached the Adriatic. From hence they were sent southward and when they came to Greece, were received first of all by the Dodoiceans. Thence they descended to the Maliac Gulf, from which they were carried across into Euboea, where the people handed them on from city to city, till they came at length to Carystus. . . . Such according to their own account was the road by which the offerings reached the Delians. . . . Afterwards the Hyperboreans when they found their messengers did not return, thinking it would be a grievous thing always to be liable to lose the envoys they should send, adopted the following plan: They wrapped their offerings in the wheaten straw and bearing them to their borders, charged their neighbors to send them forward from one nation to another, which was done accordingly, and in this way the offerings reached Delos.

Objects often believed to be of Western Asian and/or Indus origin found outside either area and in the past used as evidence for Exchange include:

1. *Metal Types.* From the depth of 18.4 feet at Mohenjo-daro in DK area (Mackay 1938b: 539, Vol. II, Pl. C, No. 4) and from Chanhu-daro (Mackay 1943:195, Pl. LXVIII, 9) belonging to the last phase of occupation were found two spiral headed pins, while two animal headed pins from Area J, Trench III at Harappa (Vats 1940:390, Pl. CXXV, 34, 36) and one from DK area of Mohenjo-daro (Mackay 1938b: Vol. II, Pl. C, 3) were recovered. Piggott (1948:26-40) has argued that these pins were imported into the Indus Valley. The presence of this generalized type at Troy II, Alaca Huyuk (Grave L), Naram Sin's palace at Brak, a mid-second millennium tomb of Mari, Hissar II, IIIc *et al.* indicated to Piggott the eastward migration of this type. Their presence in the Koban and Korca in 13th-9th century context make them at best a questionable chronological marker. Piggott's examples from Iran alone range from 4th to 2nd millennium date. We reject the evidence of spiral and animal headed pins as evidence for trade between East and West. Doubtful it is that they were even representative of an Exchange. We believe them best explained as the transmission of a generalized pin type. Among the many examples cited by Piggott no two examples are really alike. The pins from Alaca, the Caucasus, Mainland Greece, Luristan, Khurab, Kish, etc. are all similar in that animals form their head—the animals differ however as do their individual styles. We dismiss them as evidence of trade, but see in their popularity throughout late 3rd millennium Western Asia an indication of a common tradition in the manufacture of pins.

An unpublished bronze or copper knife of distinctly Harappan type was found in Hissar IIIB (Wheeler 1947a:80), while a copper axe-adze is noted from Mohenjo-daro (6 feet below the surface) and said to be paralleled at Hissar III. It has been argued that Indus metallurgy owes a great deal to that of Iran—but not, we believe, through trade in objects, but through stimulus diffusion in the development of a metallurgical technology and the production of similar functional tool types (Lamberg-Karlovsky 1967:145-62).

2. *Ceramics.* Ceramics are poor indicators for documenting the existence of trade relations but have been used to suggest cultural contacts between the Indus and Mesopotamia.

A few types of pottery have been thought to indicate contact between the Indus and Mesopotamia. The evidence is at best shaky—the selected attributes indicating typological similarities are too generalized, namely (*a*) perforated, (*b*) knobbed and (*c*) "reserved slip" ware. Perforated wares appear on several Mesopotamian sites and in Iran at Hissar, Tureng Tepe, Shah Tepe, Yahya, Bampur and Shahr-i-Sokhta. Of different shapes and date this ware in no way can be marshalled to support the existence of Mesopotamian-Indus contact or relations.

Knobbed ware is rare in Mesopotamia, Iran and the

Indus. Several sherds with knobs on the external surface at Tell Asmar and Khafajeh are dated to Jemdet Nasr and Early Dynastic III and have been paralleled to the knobbed ware in the Indus (Delougaz 1952:188). The carefully made knobbed vases of Mohenjo-daro contrast with the roughly made knobs on those of the Diyala. The general resemblance of the plastic decoration is far too vague to establish contacts between the two areas. Rare examples of knobbed ware in Iran: Shah Tepe (Arne 1945: Fig. 167, 168, Pl. XXVII, 6); Sialk (Ghirshman 1938: Vol. I, Pl. XXVIII, 6) and Yahya (Lamberg-Karlovsky 1970, Fig. 29), differ in shapes as much as those from the Indus and Mesopotamia. This type of ware cannot be used to strengthen any argument for Indus-Mesopotamian relations. Two sherds of "reserve slip" ware were found at 31.8 feet below datum at Mohenjo-daro. Mackay (1938b:184) compared these to a common "reserve slip" pottery from Kish and Ur. The evidence of two sherds indicating similar surface treatment simply cannot be used as evidence for any type of relationship or chronological contemporaneity.

Several unique objects have been used to indicate an interrelationship between the Indus and Mesopotamia. Again, they are principally Indus objects found in Mesopotamian context and would not seem to be objects of commercial value for trade. These are figurines, dice and beads.

3. *Figurines.* Three figurines found in Mesopotamia are said to compare stylistically with ones from the Indus (Dales 1968c). All three figurines come from Nippur, two were found in the "Scribal Quarter" and one from the floor of a contemporary house (TBVZ). Although one cannot deny the stylistic affinity of these figurines with those known from Harappan context (Dales 1968c) the evidence from Nippur and the Indus cities does not show an intrusive character of these figurines, thus, despite few similarities in the style of representation, they could more readily be quite independent creations.

4. *Dice.* Dales (1968c) has presented convincing evidence that one of the Indus die types (1/2, 3/6, 4/5) was actually exported or duplicated in Mesopotamia. In Mesopotamia, where dice are less common than in the Indus, the above type die has been found in the Royal Cemetery, Pit X at UR (Woolley 1955:44, 79, Fig. 7a, b), Nippur in Akkadian context (McCown and Haines 1967: Pl. 153, 11), at Tell Asmar beneath an Akkadian floor, incompletely described, and perhaps not of Indus type (Frankfort 1933:48). One cannot be certain that the die were actually imported to Mesopotamia from the Indus: stimulus diffusion of a game-type followed by independent development of die seems as likely, and is supported by the unique die type of Gawra IV (2/3, 4/5, 6/1) (Speiser 1935:82, Pl. XXXVII) which may have been copied from a southern Mesopotamian counterpart with the retention of 4 opposite 5 (the single consistent opposition on all Mesopotamian die), but varying other oppositions.

5. *Beads.* Distinctive shapes and decorative designs of several bead types have been regarded as further evidence of connections between the Indus Valley and Mesopotamia. Beads from Chanhu-daro with single, double or triple circular designs as well as ones with a figure of 8, afford close resemblances to those from Kish (compare Mackay 1943: Pl. LXXIX, Nos. 1-3, 8, 11, 15 with Mackay 1925 (29): Pl. X, 2, 3, p. 698). Similarly the rare segmented beads in the Indus (Wheeler 1968) have been compared to those more widely distributed in Mesopotamia between 3100 B.C.-1800 B.C. (Mallowan 1947:254, Pl. LXXXIV, 2; Mackay 1925: Pl. 60, 39, 40). The evidence of segmented beads tends to distort rather than clarify Indus-Mesopotamian interrelations. It seems unsafe to rely on a widely scattered bead type in both space and time for documenting Mesopotamian-Indus contacts.

These frustrating bits of information, despite large-scale excavations in the Indus and Mesopotamia, do not provide evidence for a co-ordinated effort toward mutual contacts and/or trade. Objects such as pins, dice, statuary, etched carnelian beads, stamp and cylinder seals are not an impressive list of exchanged or traded articles. Certainly it does not seem that a single class of objects were in preferential demand in either area which resulted in objects for standardized trade. We cannot turn to a single site where there is a clustering of Indus objects in Mesopotamia, or the reverse. More often than not we have seen only one to three objects of allegedly Indus derivation in Mesopotamia, and their context does not suggest a clustering in a specific area of the excavation. Perhaps, significantly, we have noted that Indus objects are found in Mesopotamia—never the reverse. The Harappan contacts with Mesopotamia, as evidenced by the scattered evidence, suggest a casual and indirect exchange. It has been argued that the Kulli people seem not to have been the exclusive middle men, i.e., merchant-venturers (Lamberg-Karlovsky 1971b).

Trade between the Indus and Mesopotamia is best seen through our model in Central Place Trade—evidenced at Bahrein and Tepe Yahya.

Central Place Trade

Locational analysis, more specifically, Central Place Theory offers a conceptual and theoretical framework relevant to a discussion of Indus-Mesopotamian relations. Fundamental to Central Place Theory is the assumption that goods and services are produced and offered at a few necessarily central points in order to be consumed at many scattered points. These central points are Central Places, their role the dissemination of goods (transshipment), or the production of goods from a given resource which they control. We have already indicated that trade, rather than a result of urbanization may have been one of the major establishing factors in the rise of urban centers. We turn to two Central Places, both important to Mesopotamian-Indus interrelationships. One, Tepe Yahya, has an early village occupation (ca. 4500 B.C.) with direct cultural continuity toward a later fluorescence (contemporary) with Mesopotamian Late Uruk sites (ca. 3300 B.C.). The fluorescence at Yahya can be attributed to its role in East-West trade and its control of natural resource—steatite, which was exported to the West (see below). At Bahrein a contemporary fluorescence appears to have been brought about by its role in the transshipment of goods rather than control of resources.

At Tepe Yaḥya in Period VA (ca. 3200-3400 B.C.) we have recovered Nal pottery, a ware long known to predate the Harappan Civilization. Period VA indicates a prosperous rural community which already makes use of local and imported resources: steatite, carnelian, turquoise, obsidian, alabaster, Persian Gulf shells *et al.* In the immediately later Period IV C we have an increase in the architectural complexity and material wealth of the site—we believe brought about by its increasing trade relations with both East and West. In IV C we have recovered from what would appear to be an administrative building (previous architecture would appear to be entirely domestic in function) Proto-Elamite tablets, Susa C cylinder sealings, distinctive cylinder seals of a type indigenous to Yahya and Uruk bevelled rim bowls. Carved steatite bowls identical in shape and motif to those found in Mesopotamia (Kish, Tell Asmar, Mari, Khafajeh, Ur, Ubaid, etc.) and at Mohenjo-daro have been recovered. At Yahya, over 1500 steatite pieces represent both finished and incompletely manufactured objects—this together with the discovery of a steatite mine some 25 km. away strongly support the manufacture and export of steatite from Yahya. We might add that the pottery represents largely an indigenous type strongly paralleled at Bampur (de Cardi 1970), Shahr-i-Sohkta (Tosi 1969) and Iblis (Caldwell 1967) but only vaguely paralleling the painted Baluchistan pottery (Lamberg-Karlovsky 1970, 1971b, 1972b, for discussion).

The work and recovery of the Late 4th millennium Proto-Elamite settlement at Tepe Yahya has obvious and important implications for our understanding of the chronological and cultural reconstructions throughout this large area of Baluchistan, the Persian Gulf and Mesopotamia (Lamberg-Karlovsky 1971b). Firstly, on chronology: we will be able through a series of radiocarbon dates to establish fixed dates to the Late Uruk, Proto-Elamite configuration in this area; Susa C, and indirectly for the Early Dynastic steatite parallels in Mesopotamia. Our dating will also establish the first understanding of the period of export of steatite from Yahya and Southeastern Iran to the West. The carved steatite bowl fragments in House V, Room 53 in DK area and House III, Room No. 76 at Mohenjo-daro can be precisely paralleled at Yahya (Mackay 1938b: Pl. CXCII; Marshall 1931: Pl. CXXXI; Lamberg-Karlovsky 1970, Fig. 21: B, D, E, F and Pl. 23, A, F). It would appear that these pieces can now be dated to the first quarter of the 3rd millennium. This together with the presence of Nal sherds in our Period V suggests that the pre-Harappan painted pottery (Nal) dates to as early as the end of the 4th millennium while the Early Harappan surely starts earlier than the reasonably supposed 2500 B.C. Certainly we cannot accept the lowering and restricting of Harappan chronology to 2300-1700 B.C. (Agrawal 1966). We would prefer to see sites as Kulli, Rana Ghundai, Mundigak, Amri, Kot Diji and the even earlier Shahr-i-Sokhta, Yahya, *et al.* of the late 4th and early 3rd millennium as directly related in a causative manner to the later consolidation of the mature Harappan. The above sites being in fact where the socio-political processes were established and later adopted in the consolidation of the Harappan Civilization.

The presence of a "Persian Gulf" type seal in Yahya IV B supports a beginning 3rd millennium date for the beginning of the Bahrein sequence, already indicated by the presence of Jamdet Nasr sherds in the Barbar

Temple (Mortensen 1970). The evidence for Bahrein as a Central Place engaged in the transshipment of goods between the Indus and Mesopotamia is evidenced from both textual and recent archaeological materials, i.e., Indus weights in the "customs house" at Bahrein, a Persian Gulf seal at Lothal (see Bibby 1969). Our strong parallels to Bampur I-IV in Period IV C indicate an end 4th millennium date for the beginning of the important Bampur sequence and a mid 3rd millennium date for its end (based on IV B parallels with the end of the Bampur sequence). Thus substantially revising the proposed chronological framework for this site (de Cardi 1970; Lamberg-Karlovsky 1970, 1971b, 1972b).

Secondly, we would like to point out that our site has no evidence for the presence of the Kulli Culture. Much has been made of and suggested for the Kulli "Merchant venturers" of the 3rd millennium (Dales 1965a: 268-74; Dales and Flam 1969). We find it indicative that at Tepe Yahya with obvious evidence for long range exchange patterns there is a lack of an identifiable Kulli element. Until we hear from the important work of Professor J.-M. Casal at the Kulli site of Nindowari it is best to call a moratorium on ascribing to Kulli the responsibility for "international trade"—a conception without evident support.

Thirdly, it becomes evident that with the distribution of Tepe Yahya, Bampur, Shahr-i-Sokhta, Tal-i-Iblis and Shahdad we have an expansive distribution of contemporary and ceramically related sites. We suggest that there is here a shared cultural "ecumene" identifiable as Proto-Elamite. Clearly, the nature of the settlement pattern, the degree of uniformity between the sites, their socio-political and economic configurations (Yahya's export of steatite, Shahr-i-Sokhta's export of lapis lazuli and alabaster, etc.) need individual attention before the above hypothesis becomes wholly acceptable (Lamberg-Karlovsky 1971b). It appears likely that a trade mechanism was established which in recognizing the value of local resources brought the Iranian highlands into a supply-demand relationship with resource-poor Mesopotamia. Mesopotamian demand for lapis, steatite and mineral ores would have provided in part the economic base for the urban development of Shahr-i-Sokhta, Yahya and Iblis. This relationship as in a feedback mechanism would have in turn aided in bringing about the developing complexity of socio-political and economic structure of the Late Uruk Mesopotamian city-state.

Fourthly, the presence of a late 4th and early 3rd millennium proto-literate settlement in distant South-eastern Iran, evidencing an indigenous and centralized socio-political structure, some 300-400 years prior to the "Early Harappan," suggests that the area of South-eastern Iran and Baluchistan may have played an important role in generating the processes which resulted in the later Harappan Civilization. Thus, we believe that at Yahya during Late Uruk and Jemdet Nasr times the natural resources which it possessed and traded both East and West contributed to its urban and concomitant socio-political development, while as in a systems feedback, a similar development took place in the resource-poor demand center of Mesopotamia. Through a similar systems mechanism we see the early development of the Harappan Culture, beginning as early as 3000 B.C. Under this stimulus of desired resources and reciprocal trade throughout Baluchistan we can see an increasing nucleation of sites (Kulli, Amri, Kot Diji, Mundigak, Shah-i-Tump, etc.) which find a culmination in the mature Harappan Civilization.

Fifthly, the role of Elam and the Elamites in Indus-Mesopotamian relations has been too long overlooked. In the 3rd millennium, situated between the Indus and Mesopotamia, was the poorly known but important Elamite Civilization. Clearly, any overland routes would have had to pass through their territory, which we now know extended eastward at least to Tepe Yahya. The relations of Elam and Mesopotamia have been well summarized by Hinz in *Das Reich Elam*:

> . . . the historian can recognize the leitmotiv of relations between Elam and Mesopotamia, one of hereditary enmity, mitigated at the same time by equally persistent economic and cultural exchanges, for Mesopotamia needed the products of the Elamite highlands, timber, metallic ore (lead, copper, tin and silver), stone (alabaster, diorite, and obsidian), semi-precious stones and also horses. The countless campaigns of the Sumerians and Akkadians against Elam were due to the need to control these important materials. At the same time they followed the political aim of warding off and keeping in check the Elamites, who were always ready to plunder the lowlands.

It is entirely possible that Direct Contact Trade between the Indus and Mesopotamia was pre-

vented by the Elamites. It is equally possible that the development of sea trade was brought about in Mesopotamia through a necessity to bypass overland routes through hostile Elamite territory. Thus, the absence of port sites of 3rd millennium date along the Iranian shores of the Persian Gulf may have also been dictated by Elamite hostility toward their establishment.

Lastly, the presence of a proto-literate site at Tepe Yahya, some 600-800 miles from the Indus Valley and 200-400 years prior to the formation of the Harappan Culture, has clear implications in generating the processes which led toward not only the development of later Indus-Elamite-Mesopotamian relations, but for the very formation of the Harappan Civilization! Thus the explosive evolution traditionally argued for the Harappan Culture (Wheeler 1968) can be seen as misleading. At such sites as Yahya, Shahr-i-Sokhta, Mundigak, Amri, *et al.* one can see the embryonic urban forms of social organization from which the later Harappan Culture was to evolve. Wheeler (1968) has pointed out that the "idea of civilization" crossed from West (Mesopotamia) to East (the Indus). One might well ask why civilization did not occur between. We believe this is a false question: for it is evident today from such a wide distribution of proto-urban sites in eastern Iran and Baluchistan, of the late 4th and early 3rd millennium, that there was an established dialectic between these resource-rich areas with resource-poor Mesopotamia on the one hand and the Indus on the other which brought about a mutually dependent parallel and contemporary process toward urbanization. The absence of a political/cultural consolidation in the area of the eastern Iranian highlands and Baluchistan may be due to the absence of a unified environment, as the essentially similar riverine environments which saw the consolidation of Mesopotamian and Indus Civilizations.

In conclusion we note that the same causal factors that create a civilization often serve to identify it. Anthropologists have used the word "intensify" to signify the heightening of cultural activity which produces this complexity (Fairservis 1961f:14). We have argued here that one of the important "intensifiers" motivating the parallel but essentially distinctive rise toward urban complexes in Mesopotamia and the Iranian highlands, and the later Harappan Culture was trade. As a working hypothesis it has gathered considerable support with the new excavations undertaken in Southeastern Iran, Sistan, Baluchistan and Turkmenia.

Of Dice and Men

GEORGE F. DALES

Professor Speiser had a keen interest in and an appreciation for international relations, both modern and ancient. He possessed an intuitive feel for the motivations behind and for the practical mechanics of the cultural, social and economic interactions between peoples and states. His archaeological and historical writings often were punctuated by seemingly modest statements concerning these complex matters —statements which have pointed the way to new and important avenues of research. Such a statement appeared more than thirty years ago in his report on the Tepe Gawra excavations (Speiser 1935). In describing a unique playing die attributed to Gawra level VI, Akkadian period, he suggested that it was "ultimately of Indian origin." When that was written the spectacularly unexpected discovery of the Harappan civilization of the Indus Valley had but recently been revealed to the world (Marshall 1931), and C.J. Gadd's fascinating description of "Indian style" seals found at Ur (Gadd 1932) was fresh from the press. Amidst the archaeological excitement and fervor of the 1920s and early thirties there was no lack of attempts at what we now call comparative archaeology, but it is a small monument to Speiser's perspicacity that he could spot correctly the "international" significance of a simple die.

Naturally, during the thirty odd years since the Gawra publication, more information has been forthcoming concerning the question of Mesopotamian-Indus relations. But unhappily the picture is far from precise. Too many of the "facts" are indeed but inferences. The purpose of this paper is to review the various types of evidence available and to introduce some new material which reflects direct contact between the Indus and Mesopotamian regions during the periods from about 2400 to about 1900 B.C.

Just what is the evidence behind the suggestions of influences and contacts between the Sumero-Akkadians of Mesopotamia and the "Harappans"[1] of South Asia? It can be classified for convenience into the following types, arranged in descending order of certitude:

(1) Actual material objects—the products of one civilization found in archaeological contexts in the other.
(2) Stylistic and typological details which suggest more than coincidental similarities between the two cultures.
(3) References in ancient written records. Inasmuch as the Harappans have not favored us with such material we can seek such information only in Mesopotamia.
(4) Suppositions concerning the oft-times assumed role of the Mesopotamians in the origin and development of Harappan civilization.

The latter point is the least tangible and the most difficult to verify. The subject has been discussed in detail several times previously.[2] Here, it will only be restated that there is no evidence to support theories of wholesale migrations of peoples and goods from Mesopotamia to South Asia. The knowledge of certain fundamental concepts and techniques—for example, writing and some metallurgical techniques—may have been learned from contact with the early Mesopotamians, or their neighbors in southern Iran, but Harappan civilization itself was a product of its own internal genius and development. It must remain a moot question as to whether *without* the initial ad-

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vances and stimuli of Mesopotamia, Harappan civilization would have sprung up at all. But we need to know much more about the mechanics of civilization-making in general before discussions of specific examples can be meaningful.

However, we are on *some* common ground with both the southern Mesopotamians and the Harappans when we observe that both riverine civilizations suffered from a common lack of basic raw materials and natural resources. There could have been no Ur or Nippur in Mesopotamia nor a Mohenjo-daro or Harappa in the Indus basin without a considerable amount of trade and exploitation of the hinterlands. A fascinating question is just how extensive the search for these basic materials was and to what degree—if any—they involved direct contact between the Near East and South Asia.

This brings us to the evidence of the ancient written word. Here, because of the as yet total absence of Harappan archives,[3] the only potential source of information is the rich store of Sumero-Akkadian cuneiform documents from Mesopotamia. There, among the tens of thousands of economic, historical and religious documents, are sporadic but tantalizing references to distant lands. But there is considerable difficulty in linking the names in the documents with actual geographic entities. There has been a special academic interest for more than a decade in locating the lands called Magan, Meluhha and Dilmun because of the economic, commercial and religious importance attached to them by the Sumerian and Akkadian scribes from about 2400 to about 1900 B.C. Mesopotamian contact with these lands involved ships and journeys of considerable distance and risk. Various locations from Egypt to India have been suggested for these three important lands. The arguments have been fully set out and discussed in a number of other articles[4] and need not be rehashed here. For our purpose, suffice it to note that many of the commodities listed as imports into southern Mesopotamia from Magan, Meluhha and Dilmun *could* have come from the southern Iran, southern West Pakistan and Indus regions. The scribal inferences alone are not sufficiently detailed to insure the positive identification of these lands. But, coupled with the bits of archaeological evidence that are slowly being collected, there is good reason to suspect that at least one of the three names refers to Harappan territory. Just how intimate the contacts were in reality is an unknown factor at this point, as is the nature of the contacts.

But let us now consider the archaeological evidence. First off is the picture formed by the geographical distribution of Harappan settlements. There is virtually no trace of Harappan penetration into the mountainous regions of Baluchistan to the west of the lower Indus Valley. There are very few archaeological hints suggesting direct relations between the Harappans and Afghanistan although there are relatively easy land routes between the two regimes which have been heavily trafficked throughout the historical periods. The only parallels which are suspiciously similar come from the French excavations at Mundigak in southeastern Afghanistan (Casal 1961c). There a small sculptured male head (Casal 1961c:76 and 255, pl. XLIII-XLV) has certain stylistic similarities to the few examples of extant Harappan sculpture from Mohenjo-daro (especially the fillet around the head). Also there is a peculiar type of ceramic object (Casal 1961c:145-6, Fig. 84; Mackay 1938b: 16ff, pl. LIV) called a trap or cage for small animals which is found at Mundigak and Mohenjo-daro and Chanhu-daro, but that is the extent of the recognizable contacts. Further north we see no appreciable signs of Harappan penetration or influence in the western Punjab and up in the direction of Peshawar and the Khyber Pass. To the north, Harappan sites are known as far as the foot of the Simla Hills (Rupar). Very important was the discovery of Harappan material at the site of Alamgirpur, about 28 miles north-east of Delhi. This is, so far, the only known site demonstrating the penetration of "mature" Harappan culture into the Ganges-Jumna basin. In the southeast we see a heavy concentration of Harappan sites in the peninsular area north of Bombay called Gujarat or Saurashtra (Rao 1963a). There are over 80 Harappan sites in this relatively small area alone but there are no signs of penetration or influence into west-central India, nor do we have even a hint of Harappan activities along the southwestern coast of India. It is to the west, along the Makran Coast of West Pakistan, that the scattered bits of archaeological evidence lead us. A University of Pennsylvania expedition in 1960 verified the presence of "mature" Harappans along the coast as far west as the Dasht Valley, the present border between Pakistan and Iran (Dales 1962b and 1962c). The fortified sites of Sutkagen-dor in the Dasht Valley and Sotka-koh in the Shadi-Kaur Valley north of Pasni must have played key roles in Harappan commercial activities. Both sites could have served as way stations for coastal

seatrade. Also, located as they are near the mouths of the only valleys which offer reasonably easy access, they commanded the gateways between the sea and inland Makran.[5]

Exploration along the Arabian Sea coast of Iran is the next logical stage in the search for Near East-South Asian connections. It would be indeed a coincidence if the modern border between Pakistan and Iran also was the western "international" border of the Harappan empire. On the other hand, if it should prove to be that no Harappan sites exist west of the Dasht Valley, it would be an interesting case study for the geographical determinists.

There are some tantalizing indications that middlemen were involved between the Mesopotamians and Harappans. They may have been enterprising merchants of the so-called Kulli culture (Piggott 1950; Dales 1965a) which inhabited large areas of southern Baluchistan. The relationships—chronological, cultural and economic—between the Harappans and these enigmatic Kulli people remain one of the most fascinating problems in South Asian archaeology. The extremely limited amount of archaeological evidence available so far suggests that Kulli culture was at its height just prior to the advent of Harappan civilization and that there was some overlap with the Harappan period. Just how much overlap there was is a moot question. Recently, pottery having distinctive Kulli and other Baluchistan characteristics, was excavated from burial cairns on the island of Umm an-Nar off the coast of Abu Dhabi in the southern Persian Gulf.[6] This attested presence of the Kulli-ites in the Persian Gulf region suggests that they may have been intimately involved in the assumed communications between the Harappans and Mesopotamians. But there may have been other peoples involved. Danish excavators (Glob and Bibby 1960; Glob 1958a and 1958b; Bibby 1958b) have since 1952 been uncovering the remains of a unique culture in the Persian Gulf which was at least partly contemporaneous with the time span concerned in the subject of this paper—namely from about 2500-1900 B.C.

Especially significant among the Persian Gulf finds are the stamp seals of a style unique to this region (Bibby 1958a; Gadd 1932; Wheeler 1960a: 90-3). Circular in shape, with a pierced boss on the back side, these seals display a few iconographic details reminiscent of Kulli painted pottery, but most importantly, some of them are engraved with Harappan script. Because of their distinctive shape and style, Wheeler has dubbed them generically "Persian Gulf seals" (1958).

A happy discovery was made recently, not in the field but in the vast collection of cuneiform documents in the Yale University Babylonian collection. A tablet representing a mercantile agreement of the Larsa period, dated precisely to the tenth year of Gungunum, King of Larsa, and bearing the impression of a typical "Persian Gulf" seal has been published by Drs. Hallo and Buchanan (Hallo and Buchanan 1965; Buchanan 1967). This single bit of evidence provides a crucial anchor point for dating the Persian Gulf finds. But more than that, it gives important inferential dating evidence for the Harappans. A Persian Gulf-type seal was discovered in western India at the Harappan harbour town of Lothal (Rao 1963b and 1965b). Unfortunately it was a surface-find but given the weight of the other indications of Harappan—Near East contacts during the centuries before and after 2000 B.C., it is more than probable that the Lothal seal belonged to the period of Harappan occupation. If nothing more, its very presence in India does furnish us with some concrete evidence of communication between the two regions. Less tangible, but equally as suggestive, is the comparative evidence of stylistic and typological similarities between Harappan and Mesopotamian artistic and decorative elements. Sir Mortimer Wheeler (1960a:90-3) has presented a comprehensive description of the most significant of these similarities—e.g. certain distinctive types of beads, the trefoil pattern, the spread-wing eagle motif, the representation of humans disguised as animals, and the "Gilgamesh motif" which depicts a savage looking man standing triumphantly between two upright animals.

Granted, there are profound dissimilarities also—e.g. in the type of seal (cylinder as opposed to stamp), architecture, sculputre, etc.—but these are what mark the crucial differentiation between separate societies and cultures. The scattered *similarities* are the more difficult to explain. In this particular instance there are enough distinctive ones to suggest convincingly that some degree of mutual contact must have existed. It is most significant that those similarities which do exist can be dated from the Mesopotamian end between the latter part of the Sumerian Early Dynastic period and the end of the Larsa period, just where the other types of dating evidence point (Agrawal 1964; Dales 1965a; Stuckenrath 1967).

This brings us finally to the evidence suggested

by the title of this paper. Mention has already been made of the terracotta die "of Indian origin" discovered by Professor Speiser in his Tepe Gawra excavations. Its number dots are arranged consecutively on opposite sides so that 2 is opposite 3, 4 opposite 5, and 6 opposite 1.[7] Speiser's "Indian" attribution for the Gawra die suggested a basic synchronism between the Mesopotamian Old Akkadian period and the "mature" Harappan period of South Asia.

Since Speiser's publications, only a few other cubical dice have been reported from Mesopotamian sites. The most significant discovery was that of "broken clay dice" in a "hoard" found in a pottery vessel buried beneath an Akkadian period house floor at Tell Asmar in the Diyala region of central Iraq (Frankfort 1933). It was in this and other "hoards" from Akkadian houses at Tell Asmar that etched carnelian beads, bone inlays, stamp seals, a cylinder seal, and a distinctive type of knobbed pottery—all these with strong Harappan stylistic parallels—were found (Frankfort 1933:47-53). Two dice were found at Ur. One of bone (Woolley 1955:44, Fig. 7a) has lightly incised dots, now partly obliterated, but whose order is certainly not like modern dice. It was dated by Woolley to the 1st Dynasty of Ur (i.e. Early Dynastic III). The second die is of grey clay (Woolley 1955:44 and 79, Fig. 7b). Its dots are arranged 1 opposite 2, 3 opposite 6, 4 opposite 5. It was found loose in the soil of Pit X in the Royal Cemetery and could date to anytime from Early Dynastic III to the Ur III period. A single die of baked clay with 1 (or x) opposite 2, 3 opposite 6 and 4 opposite 5 was found at Nippur but in a context too late to be relevant here (Kassite) (McCown and Waines 1967: pl. 153, 11). Games were certainly popular throughout Mesopotamian and Near Eastern history but the use of cubical dice seems to have been a rare and late innovation.[8] The archaeological evidence—albeit scanty—directs our attention instead to South Asia and to the Harappan period for the possible origin of the cubical type of dice (Brown 1964). Like the Tepe Gawra die, many of the Harappan examples are made of terracotta and the number dots are arranged in non-modern order.

Sir John Marshall in his Mohenjo-daro excavation report states that "many" dice were found at the site, although he publishes only four examples (Marshall 1931:551-2, pl. 153, f. 7-10). They are made of terracotta and the opposite numbers are arranged 1/2, 3/4, 5/6. Ernest Mackay, who continued the Mohenjo-daro excavations (Mackay 1938b:559-60) reported that dice were found at "all levels"[9] of the Harappan city. Published examples include three terracotta dice, one with the opposite dots arranged 1/3, 2/4, 5/6 (Pl. CXL, 19); one with the dots arranged 1/2, 3/4, 5/6 (Pl. CXL, 20) and another with identically arranged dots but inlaid with tiny beads (Pl. CXLII, 84). Three stone dice are also published: one of yellow agate with dots arranged 1/2, 3/6, 4/5 (Pl. CXL, 63); one of white limestone with dots arranged 1/3, 2/5, 4/blank (Pl. CXLII, 85); and one of light grey stone with dots placed 1/2, 3/5, 4/6 (Pl. CXLII, 86). From the Harappan period levels at the city of Harappa itself, seven examples have been published (Vats 1940: pl. 120, f. 46-8 and 51-4), two of stone, four of terracotta and one of faience. Four of these have the opposite dots arranged 1/2, 3/4, 5/6; two have them placed 1/2, 3/5, 4/6; and a unique terracotta example has the dots arranged in modern order—i.e. with the opposite dots adding up to seven.

At the northeasternmost Harappan site, Alamgirpur—not far from Delhi—one terracotta cubical die was found in the limited excavations (Archaeological Survey of India 1959:52, pl. 62). The photograph shows three sides of the die with the number dots two, four and six but the remaining three sides are not described. Similarly, near the southeastern extremity of the Harappan domain, a single terracotta die was uncovered in the Harappan levels at Lothal (Rao 1962), the presumed seaport site where the "Persian Gulf" stamp seal was found. Its dots are arranged 1/2, 3/5, 4/6. As for the two remaining major Harappan sites which have been subjected to excavation, no cubical dice were published from Chanhu-daro (Sind) (Mackay 1943) or from Kalibangan (Rajasthan) where excavations are still in progress. Thus at the two major Harappan cities alone, an overwhelmingly larger number of cubical dice has been found than has been uncovered in the numerous and extensively excavated contemporaneous sites in all of Mesopotamia.

From the sportive lightheartedness inferred by these gaming pieces, we move to a different class of objects whose use and significance is more enigmatic. Whether they were of religious significance, as their features seem to suggest, must remain an open question until the unlikely day when we can begin to understand something about Harappan religious practices. I am referring specifically to a most distinctive type of male terracotta figurine. These figures, fashioned in-the-round, depict a nude male body, perhaps ithyphallic,

with extremely obese stomach, prominent buttocks, shoulder-holes for the attachment of movable arms and stubby tail. Most of the examples have animal-like faces and deep holes in the navel and rump. Now there is absolutely no stylistic similarity between the majority of Near Eastern and Harappan clay figurine. Thus to find examples of such a peculiarly distinctive type of representation in both areas suggests strongly some common contacts—or at least common awareness.

Hundreds—perhaps thousands—of clay figurines have been excavated from Mesopotamian sites. They are well enough documented so that a reasonably comprehensive classification of them—by type, style and period—has been possible. Figurines of "foreign" origin or inspiration can be recognized with reasonable assurance. The novel type of nude male figurines under consideration here is emphatically not a characteristic Mesopotamian creation. Neither *male* nudity, male obesity, nor animation are found among Sumero-Akkadian figurines of this date. On the other hand, the practice of combining human and animal features was common throughout Mesopotamian history. This will be further discussed below.

The Mesopotamian examples, only three in number, come from the current series of excavations at the holy city of Nippur in southern Iraq (McCown and Haines 1967). All were found in the "Scribal Quarter" part of the city—one was a surface find but two of them were discovered in private houses dated definitely to the Sumerian Ur III period (approx. 2100-2000 B.C.)

The most complete of the three examples (Fig. 15.1) was found on the floor of a house in the fifth level of the so-called TB area of Nippur.[10] It is very crudely executed. The grossly protuberant belly has a deep hole at the navel. There is a deep hole in the arms and another down through the top of the head (for attaching a headdress?). The shoulders, which are far out of proportion to the rest of the body, are pierced through to allow the attaching of movable arms. The penis is broken but marks on the belly suggest that the figure was ithyphallic. A stubby tail is crudely but clearly indicated. The face is, fortunately, almost intact. It is that of a bearded human male.

The second dated figurine from Nippur[11] was also found in association with an Ur III house in the TB area. The legs and head are missing but otherwise it is unmistakably on the same type of fat male figurines. The shoulders are pierced for movable arms. Unhappily the published photograph is too unclear and

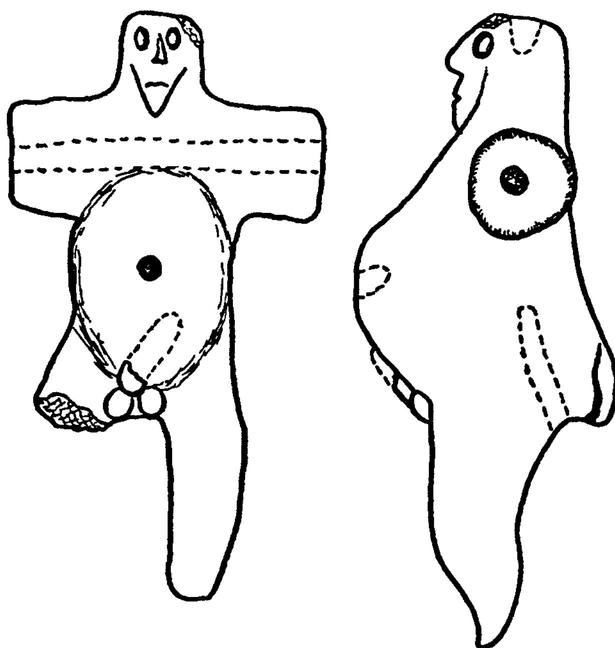


FIGURE 15.1. *Nippur.*

the description too brief to furnish us with further details. One unusual feature is the depiction of bands crisscrossing the chest and belly.

The third and remaining Nippur example[12] was found on the surface of the same area of Nippur and is certainly of the same type. Only the torso is preserved but the genitals are visible in the photograph as is the deep navel hole.

Thus we are confronted with a small group of atypical figurines in southern Mesopotamia, found in contexts dating to the period of presumed Sumerian international seafaring activities. Is it merely coincidental that we find almost identical fat male figurines at Harappan sites where they are typologically and stylistically at home? The basic concept of animated figurines with movable heads and arms is widely manifested among Harappan figurines as is the popularity of grossly exaggerating the roundness of the human body. There is a large corpus of Harappan figurines, both male and female, whose bodies are blown up into round balls.

The most important Harappan example is from Chanhu-daro in the southern Indus Valley (Fig. 15.2).[13] This rather carefully made figurine is intact except for its head and lower legs. Also, the end of the penis is missing but enough remains to suggest that the representation is ithyphallic. The body profile is virtually identical to that of the Nippur example. A

stubby tail is depicted and the heavy shoulders are pierced for movable arms. It differs from the Nippur example in that the navel and arms are not pierced. Unfortunately the head is missing but traces of a red painted "necklace" remain.

Quite a few fat male figurines have been published from Mohenjo-daro. They are of added importance because several of them, while having human bodies, also have animal heads and tails.[14] Both Marshall and Mackay interpreted these objects as representing some sort of abortive female figures. Of one, Marshall (1931:549, pl. 153, f. 38) says it is "obviously represented as pregnant and the extreme exaggeration of the buttocks suggests steotopygy." Mackay (1938b:294), in describing his finds at Mohenjo-daro, states that "they were obviously intended to represent human forms with animal heads .... " He too felt that the swollen bodies represent pregnancy. Now, with more examples available for study—especially the well preserved Chanhu-daro and Nippur ones—we can say with a high degree of certainty that some of the objects are basically human males with some animal attributes. Just as in Mesopotamian art, the combining of human and animal attributes was an important aspect of Harappan iconography, seen not only in the figurines but in the scenes engraved in the stone stamp seals. There one sees curious groups of human figures wearing horned headdresses and long animal tails. Its

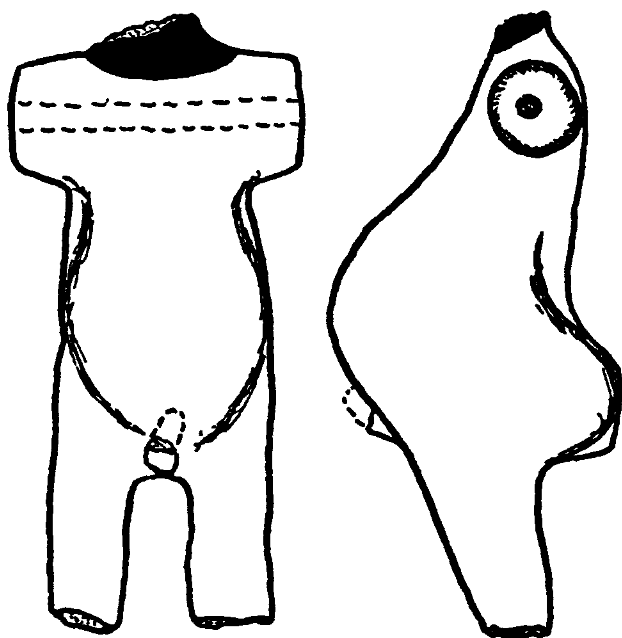


FIGURE 15.2. *Chanhu daro.*

purpose and significance for the Harappans will probably never be known to us. Even in Mesopotamia with its voluminous surviving corpus of religious and mythological written records plus its abundant artistic remains, the true significance of human-animal representations is still far from modern understanding. For example, the Sumerians often depicted humans dressed up as animals but performing human activities such as playing musical instruments. Such representations are seen on stone religious plaques of the Early Dynastic period and most strikingly in the inlaid scene on a wooden harp from the so-called "Royal" tombs at Ur. Even the magnificent gold bull's head surmounting the harp is sporting a heavy curly beard of lapis lazuli. In this matter it would be presumptuous to insist on any definite relationship or influence between the Harappans and Mesopotamians, but in light of the other evidences for mutual contacts during this period from about 2500-1900 B.C., the possibility cannot be overlooked.

The ultimate significance of inquiries such as this is not just to play intellectual games with bits and pieces of ancient castoffs and debris. The aim, very seldom attainable, is to reconstruct the life and times which produced the tattered remains we so arrogantly "discover." This is especially true with objects such as we have discussed in this paper. The objects themselves, when treated collectively, make a convincing case for Near East-South Asian contacts. But what we would really like to know is the nature and extent of these relations. Just how cognizant were the citizens of each region of the peoples and culture of the other? What degree of *dependence*—if any—was involved?

The importance of seafaring activities for both the Harappans and Mesopotamians is strongly indicated, if not proved, by the archaeological and written evidence. On the Harappan end we have only the mute archaeological indications, including the distribution of what appear to have been major seaport towns. The Mesopotamian written records, on the other hand, point out specific goods and materials which were being imported from some distant lands—probably in part at least from the Harappan region. Partial explanation for why virtually no Mesopotamian objects or products have been found in Harappan sites comes from these Sumero-Akkadian documents. Because southern Mesopotamia has no raw hard materials, it was necessary to import all the wood, stone and metals for the needs of the civilization. "International" trade was therefore a vital

concomitant of the practical endeavors of the southern Mesopotamians. In return, the Mesopotamians had only soft and consumable items to exchange. Among these were garments, wool, "perfumed" oil, and leather products—items which have left no trace in the archaeological record.

Finally, potentially significant chronological "coincidence" should be noted. Or was it a coincidence? From the Mesopotamian economic texts we learn that by the end of the Larsa period "international" trade came to a virtual standstill. This is usually explained in terms of internal Mesopotamian difficulties. But the end of the Larsa period is also the lower limit for the finding of Harappan objects and influences in southern Mesopotamia. And this is apparently, on the basis of the carbon-14 chronology for South Asia, the end of the mature Harappan period, precipitated, we think, by natural disaster and floods. Were these events which just coincidentally occurred in these distant regions or was there some very serious effect of the decline in one area on the fortunes of the other? Could the unexpected disasters in the southern Indus Valley have deprived the Mesopotamians of vital raw materials for the maintenance of their complex, but highly vulnerable, empire? These questions can probably never be satisfactorily answered, but they are worth asking as long as we are engaged in the search for a better understanding of the mechanics, the advantages, and the liabilities of international relations.

The continued discovery of such mundane objects as dice and figurines alone can provide the material verification for such inquiries.

NOTES

[1]The name "Harappan" is merely a modern convenience derived from the present day name of the site in the Punjab where the Indus Civilization was first recognized. We know not a single word of their language, much less what they called themselves.

[2]See especially the following: Wheeler (1966 and 1968); Fairservis (1961f); A. Ghosh (1965a) and Dales (1965a).

[3]That the Harappans knew the art of writing is generally agreed but the only extant examples are short, as yet undeciphered, inscriptions on stamp seals, pottery and other small objects. No hint of a library or even of a lengthy text has yet been found in a Harappan context. Perhaps their main corpus of writings was put onto soft materials such as leaves or wood which have perished. One can speculate that if there is any validity to the theories of commercial and cultural contacts between Harappans and Mesopotamia, there may be undiscovered archives containing bilingual records on clay tablets. In light of the negative evidence so far from excavations at major sites such as Harappa, Mohenjo daro, Chanhu daro and Lothal, I would suggest that such records might be found alleged Harappan seaports such as Sutkagendor and Sotak koh. See Dales (1962b and 1962c)

[4]See especially the following: Oppenheim (1954); Leemans (1960b); Birot (1960); Kramer (1963a and 1963b, 1964); Mallowan (1965); Buchanan (1965, 1967); Gadd (1963), Gershevich (1957); Jacobson (1960).

[5]Their present location, some miles from the seacoast, is apparently a result of geomorphological changes in the coastal region since Harappan days. That such changes might even have been partially related to the decline of Harappan prosperity is a possibility (Raikes 1965c).

[6]See the Danish archaeological *Kumul* for yearly reports on this work for 1962, 1964 and 1965. See also Bibby (1964).

[7]None of the dice described here—except one—has the dots arranged in the order of modern dice on which opposite numbers always add up to seven

[8]In Egypt they were not common until Graeco-Roman times, although isolated examples have been found at Thebes and Amarna dating to the New Kingdom (mid 16th century B.C. at the earliest). See Hayes (1959:405)

[9]This means, in fact, only approximately the upper two-thirds of the total 75 feet of occupational remains. Recent tests at the site have shown that at least 25 feet of the lowest and earliest levels of the city are below the present ground water level and have been untouched for practical purposes by the excavators.

[10]Incorrectly and incompletely described as "a seated figure: perforated from anus through top of head, holes in arms, top of head chipped at left front" (McCown and Haines 1967:128-129). It is now in the University Museum, Philadelphia, no. 53-11-69.

[11]See McCown and Haines (1967:94 and pl 144, f.8) where it is grouped with chariot models— ". . . suggestive of a chariot because of its 'axel', but it could also be a remnant of a manikin with movable arms and head."

[12]McCown and Haines (1967:89, pl. 128, f. 7). Described only as a "seated figure."

[13]Mackay (1943:166-167, pl. 59, 2). Part of the Boston Museum of Fine Arts collection presently on loan to the University Museum, Philadelphia.

[14]Marshall (1931: pls. 95, f. 24; 118, f. 15; 153, f. 38 and pp. 344 and 549). Mackay (1938b:pls. 74, f. 13; 78, f. 3, and f. 12; 81, f. 5, 8 and 14; and pp. 294-295 and 314-315). They are apparently not common at Harappa but some of the "pregnant women" may in fact be this type. See for example Vats (1940: pl. 76, f. 28-30).

A Dated Seal Impression Connecting Babylonia and Ancient India

BRIGGS BUCHANAN

There has long been evidence for some kind of relationship between the cities of the Harappan civilization, in the Indus Valley, and those of Babylonia during the latter part of the third millennium B.C. and into the second. Most of this evidence consisted of seals of Indus type found in Mesopotamia. More is now known about the Harappan cities, thanks to recent excavation at Mohenjo-Daro (Dales 1956b), but a great deal remains to be learned, not least about the nature of the relationship between these cities and the centers of Mesopotamian civilization. The period when the Harappan cities flourished is still uncertain. In the main the date has so far depended on comparative archaeology, with little agreement among the authorities. Now the Yale Babylonian Collection, with its inexhaustible riches, has yielded a document which may eventually help to give the Harappan civilization and its successors a firmer chronological basis.

The catalogue of the seals in this collection, being prepared by the author, will also include a large selection from the thousands of seal impressions on clay tablets. In the course of work on the catalogue, impressions of an unusual seal were found on an inscribed tablet dated to the tenth year of Gungunum, King of Larsa, in southern Babylonia—that is, 1923 B.C. according to the most commonly accepted ("middle") chronology for the period.

The design in the impressions (Fig. 16.1) closely matches that in a stamp seal (Fig. 16.2) found on Failaka Island in the Persian Gulf, west of the delta of the Shatt al Arab, which is formed by the confluence of the Tigris and Euphrates rivers. The seal is one of 169 examples, apparently all of the same distinctive group, which were found on Failaka Island by a Danish expedition in the Persian Gulf area. Similar seals, in much smaller numbers, came from the island of Bahrein, farther south in the Gulf. Some were also found earlier by Sir Leonard Woolley at Ur, in Babylonia. One important example comes from the Indian excavations at Lothal, near the Gulf of Cambay, north of Bombay. From this evidence it is quite clear that the designation "Persian Gulf seals" is an entirely appropriate name for the group.

As far as can be determined from the relatively few so far published (seventeen in all), the seals show great uniformity of style. They are recognizable by stylistic traits such as deep, sharp engraving, human bodies broadly cut, with linear outlines and rounded forms often exaggerated, human heads usually made up of two vertical lines from which horizontal lines project, animal eyes rendered as a dot in a circle, other design elements often hatched. Uniformity also prevails in the shapes. The seals are circular in contour and thick enough for easy handling. Their most distinctive feature is a low-domed boss covering much of the flat back. The boss is marked by parallel grooves (usually three) on either side of which are two dot-in-circle devices. The material of which they were usually made seems to have been white-coated steatite, as is largely true of the Indus seals.

The designs on the published Persian Gulf seals show a great variety of subject matter. Most of the repertory seems to derive from Mesopotamian glyptic art, but usually in a modified form. The common Neo-Sumerian motif, two human figures flanking a crescent standard, has been made continuous with a third human figure and an animal by the grasping of the animal's horns. That the first two figures appear to be standing on the animal may be the result of the circular contour of the seal. Some motifs, like the bucrania (ox

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heads) in Figs. 16.1, 2 may be indigenous to the area of the Persian Gulf, though perhaps remotely derived from prehistoric Near Eastern iconography. Indian influence can be detected in the animals so frequently depicted, for they look like corrupt versions of Indus engraving.

A clear case of Indian influence may be found in a seal from Ur (Gadd 1932: Pl. II, no. 12), since all the elements in the design except the stars resemble Indus hieroglyphs. It is true that motifs like the water carrier shown here do appear, though rarely, in Near Eastern glyptic—an Egyptian cylinder seal in so-called "button seal style," dating from about the twenty-second century B.C. (Yale Babylonian Collection Seal no. 12776). However, it seems more likely that the design on the seal in Gadd (1932: Pl. II, no. 12) is a decorative treatment of hieroglyphs, which may have been familiar to the seal-cutter from Indus seals imported through trade. Because of the context in which it was found at Ur—debris of Kassite (?) date—this seal has been used as evidence that the Indus civilization ended about 1500 B.C. The date of the Persian Gulf seals (of which this is an unmistakable example) shows, however, that this argument is no longer tenable.

The uniformity in style applies particularly to the published seals from Failaka, despite their wide range of subject matter, and suggests that all of them could have come from the same workshop. Those from Bahrein and from Ur are nearly as consistent in style, except for three variants from Ur which may be local imitations. Such uniformity indicates that the seals of the group were produced during a comparatively short period. The date of the tablet at Yale University suggests that this period may be approximately the second half of the twentieth century B.C. Contemporary cylinder seals in Babylonia show a simplified decadent style, while in the surrounding regions they are schematic. This stylistic pattern agrees with that of the Persian Gulf stamp seals and tends to confirm the proposed period of their production.

That these seals were by-products of the Persian Gulf trade is indicated not only by the impressions on the Yale tablet but also by the number of seals found on the islands in the Gulf, where entrepôts of that trade must have been located. Trade on the Persian Gulf was in existence well before the time—about 2350 B.C.—when Sargon, the first Akkadian king, referred to ships from or destined for Melukhkha, Magan and Tilmun (Dilmun) at his wharves. These places continued to be the principal centers toward which the Persian Gulf trade was directed. The trade is quite well attested on tablets from Ur dated late in the Third Dynasty of Ur (around 2000), when it apparently was centered at Magan. It is even better documented on other tablets from Ur (from about 1900 and from about 1800), belonging to various kings of Larsa. At this time the trade was centered at Tilmun.

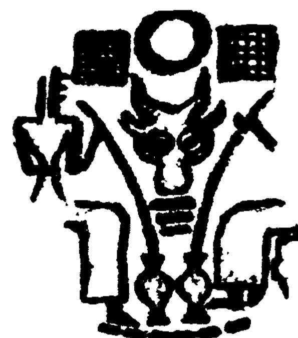


FIGURE 16.1. *Drawing of a seal impression on the Yale tablet.*

The location of the places cited by Sargon has long been the subject of controversy. Comparing the number of seals found on Bahrein and Failaka islands suggests the possibility that they were respectively the sites of Magan and Tilmun, and there is some slight documentary evidence for locating Melukhkha on the Persian side of the Gulf. Late documents, tending to equate Melukhkha with Ethiopia and Magan with Egypt, are not relevant to our problem. Tilmun, however, was the name given to a kingdom south of Bit Yakin—an Aramaean state often at war with the late Assyrian kings, the focus of which seems to have been the "Bitter Stream," probably an inland lake similar to, but perhaps even bigger than, the one of our era in southern Iraq. The records imply that Tilmun lay south of this on the mainland at the head of the Gulf, in which case it would almost certainly have included Failaka Island and may even have extended as far as the latitude of Bahrein. A similar situation could have existed in the earlier period with which we are dealing. Three cuneiform inscriptions naming Inzak, the god of Tilmun, were found on Failaka and, a long time ago, one on Bahrein. Combined with the evidence of the seals, this makes it virtually certain that Failaka can be equated with Tilmun, or at least was an important part of it. It is also implied that Bahrein had some connection with Tilmun, perhaps as another part of it. However, this does not exclude the possibility that Bahrein had its own name, Magan.



FIGURE 16.2. *Design on a stamp seal from Failaka Island, Persian Gulf.*

The Persian Gulf seals are then to be associated with the final (Larsa-Tilmun) phase of the Gulf trade. The single example of these seals so far found in India argues for some trade connection with the Persian Gulf at this time, even if intermittent and indirect. Unfortunately, the Lothal example was a surface find, while the site from which it came had both Indus and post-Indus levels. Therefore, although the seals are firmly dated on the Babylonian side, as yet they are of little help in determining a more precise Indian chronology.

Some indirect evidence, however, is available. In approximately Akkadian times a number of Indus stamp seals appeared in Mesopotamia, probably by indirect trade, since no objects clearly of Mesopotamian origin have been found at Indus sites. These stamp seals have a flat back with a small, rather high-domed boss, which is divided by a single deep groove. Some have the square contour that was overwhelmingly favored in the Indus civilization, while others are circular but with designs in the same style as the square seals. These round seals were perhaps made especially for trading purposes, since few have been found in India.

A very few circular seals from Bahrein and Ur have a back of Indus type combined with designs of a mixed character in which degraded Indus motifs predominate. Few as they are, they perhaps represent an early stage of Persian Gulf glyptic—a suggestion for which some stratigraphic evidence was found on Bahrein. Possibly they belong to the Ur III-Magan phase of the trade. In any case, it would seem desirable to distinguish them from our group by calling the latter the Tilmun seals.

In addition to the stamp seals, some cylinders with designs of Indus type are also roughly of Akkadian date. One such cylinder came from a more precise archaeological context than is recorded for any of the other Indus seals from Mesopotamia (Frankfort 1955: no. 642). It is cited as a "Late Agade" find along with many Late Akkadian and a few post-Akkadian seals. None of the other Indus imports had a findspot certainly earlier than this. The Mesopotamian evidence, therefore, does not require a date for the mature Indus civilization much, if at all, before the twenty-third century B.C. Since the mature phase was relatively homogeneous, very widespread and apparently quite stable, it would be surprising if it lasted much longer than three hundred years. After all, the longest period of relative stability on record, that from the Fourth into the Sixth Dynasty of Egypt, lasted only about four hundred years. It is therefore possible that the mature Indus phase ended about 2000 B.C.—a date also indicated by some recent Carbon-14 tests. It might seem that such a date best fits the drastic change in the shape of the Tilmun seals from that of their presumed Indus model, as well as the corrupt reflection of the Indus style which may be detected in them. However, these factors might be otherwise explained. It appears that the chronological problem will be solved only by more excavation, or more certainty about Carbon-14 dates, or perhaps by another happy find in existing collections.

A "Persian Gulf" Seal from Lothal

S.R. RAO

Lothal is an important harbour-town of the Indus Civilization at the head of the Gulf of Cambay on the west coast of India (Fig. A). During the recent excavations there, a circular steatite seal has been found which is neither wholly Indian nor Sumerian in workmanship (Plate X). On the other hand, it closely resembles the seals from the Persian Gulf islands found by the Danish expedition led by Professor Glob and Dr Bibby. Sir Mortimer Wheeler has named them "Persian Gulf" seals which, according to him, "appear to have been made at the various entrepôts (such as Bahrain itself) of a cosmopolitan Persian Gulf trade of the kind which has been analyzed by A.L. Oppenheim from Larsa tablets" (Wheeler 1958). Commenting on these seals, the late Col. D.H. Gordon wrote: "The problem of Bahrain is a very interesting and important one, and it is possible that these seals may help to solve it. Some day such seals may come to light in India, but so far they have not; Bahrain may have been Dilmun and it was almost certainly an entrepôt on the trade route to India, and so it is possible that seals of this kind were carried on to the Indus or to ports in Kathiawad and will some day be found in those localities, though this will not necessarily make them Indian or even of Indian style" (Bibby 1958; Gordon 1958b, Wheeler 1958). The hope expressed by Gordon has now been fulfilled by the discovery of a "Persian Gulf" seal at Lothal, thus providing the first real evidence of trade contacts between India and the Persian Gulf. The material of which the Lothal seal is made is steatite of a light grey colour with a creamy surface. Its diameter is 2.25 cm. and the thickness at the centre 1.2 cm. At the back is a perforated boss covering almost the entire surface and divided by triple lines between four circlets with a central dot. On the face are two jumping goats or gazelle-like animals looking behind and flanking a double-headed dragon. Both in motif and shape this seal is entirely different alike from the normal square seals of the Indus Civilization and from the Sumerian cylinder seals. It bears no script of any kind. On the other hand, it has a resemblance to the circular seals of steatite found in the excavations at Barbar and Ras-al-Qala in the island of Bahrain in the Persian Gulf (Glob and Bibby 1960). Comparable circular seals found in the excavation in Failaka, a little island near Kuwait, are assigned by Dr Bibby to the Sargonid period. They are identical in almost every detail, except for the variation in size, with the Persian Gulf seal from Lothal, and, in so far as a single "document" is valid, indicates that the Lothal merchants traded with the Persian Gulf merchants in the latter half of the third millennium.

The carbon 14 date for a charcoal specimen from the late levels of the structural phase III of Lothal "A" is 3985 B.P. ± 115, i.e. 2023 B.C. ± 115. The sudden expansion of the village settlement into a well-planned port-town with a large dock and warehouse facilities took place in the previous phase II A of Lothal "A". This expansion continued in phase III, when the town reached its maximum prosperity. The "Persian Gulf" seal is unfortunately a surface find, but is most unlikely to have reached Lothal earlier than phase IIA and may reasonably be ascribed to phase III (2000-2200 B.C.).

A few seals of foreign type are also found in the Indus valley itself. Among them the following may be recalled (Wheeler 1960a:84):

(*i*) A seal from Mohenjo-daro with two crouching antelopes reminiscent of seals from Elam.

(*ii*) A lozenge-shaped seal with an eagle and snakes on one side and a cross on the other from Harappa.

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The head of the eagle is turned to the left and its wings are spread as in the case of the bronze eagles from Susa and Tell Brak. The Tell Brak eagle is dated *c.* 2100 B.C.

Equally the circular steatite seal from Lothal under discussion could not have been native to India. Other evidences also from Lothal such as a bronze amulet, a copper ingot and terracotta sealings with geometric designs similar to those on seals from Tell Brak, Susa, Ur and Sialk suggest trade with Mesopotamia during the Sargonid period. The sealings are as follows:

(*a*) A terracotta sealing, No. 1292, from Lothal used for sealing a jar bears two impressions of a square seal with multiple parallel lines drawn in a swastika pattern and is similar to a seal from Tell Brak (Mallowan 1947). At Harappa also a terracotta sealing bearing a square swastika in multiple lines is found (Vats 1940: pl. 95, no. 385). This type of swastika is unusual in the Indus valley but more common in Mesopotamia. Circular seals with similar swastika motifs are found at Ur,[1] and Alisar[2] has also yielded a square seal similar to the Lothal sealing. From Sialk (Ghirshman 1938: pl. 87, S. 17) comes a circular seal with a swastika in multiple lines.

(*b*) The terracotta sealing, No. 1833, from Lothal bears a compartmented square design similar to that on seals from Giyan (Conteneau and Ghirshman 1935:38, no. 13). Compartmented and concentric squares occur also on a sealing from Harappa (Vats 1940: pl. 95, no. 395), but the type is rare in the Indus valley.

It may be remarked that most of the sealings from Lothal indicate usage on packages wrapped in rush or bamboo-mats and secured by strings. In some cases impressions of twisted cords tied into knots are also observed. Of the Indian exports carried in this trade there is less evidence, but they certainly included ivory and shell objects and beads of gemstones, and probably cotton or cotton goods. The raw material for beads came from Mehhgam and Bhagatrav, the two Harappan sites on the Narmada and Kim rivers respectively. A bead-factory with a kiln has been found at Lothal, and a large quantity of beads of gemstones in various stages of manufacture, three bronze drills and other accessories of the lapidaries have also been recovered there. Ivory-working was another Lothal industry, as is evident from the ivory tusk and ivory objects found in the excavations. It is further suggested that elephants lived around Lothal, not only because a leg-bone of an elephant has been recovered at Lothal itself but also because Kautilya (Sastry 1924: Ch. 23, 50) refers to the short stature of the elephants of Saurashtra. A terracotta sealing from Lothal bears on the back the impression of a piece of cloth of plain weave; and how extensive was the trade in cloth is indicated by the discovery at Umma near Lagash (Wheeler 1960a:91) of the imprint of an Indus seal from a bale of cloth. Shell was probably another commodity exported from Lothal. The rocky coast near Porbandar and Jamnagar is one of the sources of Indian chank-shell. Other varieties were imported into Lothal from South India and processed for export purposes. Two workshops of shell-workers in the bazaar street of Lothal, as also the large quantity of finished and unfinished objects of shell including bangles, inlay, etc., found here, suggest that Lothal was an important shell-working centre. Dentalium beads and shell-inlays found at various sites in the Euphrates-Tigris valley may be supposed to have come from Lothal and the Indus valley cities.

Copper and copper alloys had to be imported into Lothal from West Asia in the form of ingots for making tools required by carpenters, smiths, bead-makers, shell-workers and fishermen. Neither the alluvial plains around Lothal nor the hills of central Saurashtra produce any copper ore or tin. There is no evidence to show that the Khetri copper-mines of Rajasthan were worked in prehistoric times. Furthermore tin is nowhere available within easy reach of the Indus valley. Even the Delwara and Devbari mines may not have been worked by the Copper Age settlers of Ahar[3] in South Rajasthan. A bun-shaped ingot of copper found at Lothal provides a possible clue to the import trade in copper; ingots of similar size and shape are found at Mohenjo-daro (Mackay 1938b: pl. 132, nos. 37-8) and Susa. Two workshops of coppersmiths have also been identified at Lothal, one at the north end of the town and the other in the centre of the bazaar street. At the north end, a furnace and a small quantity of copper slag in two heaps have been recorded. A brick-enclosed furnace, a stone anvil and terracotta crucibles found along with a hammerstone and copper tools in a coppersmith's workshop in the bazaar street confirm that copper tools were prepared locally. Since several Indus objects such as etched carnelian beads and shell and terracotta gaming-pieces are found at Susa, whilst Susan objects such as a bronze amulet with couchant bull and painted pottery with Susan motifs are found at Lothal, it is likely enough that Lothal imported also bun-ingots of copper from Susa.

Indus seals and other knick-knacks could not have

travelled to Ur, Kish, Lagash, Tell Asmar, Brak, Susa, Diyala and further beyond but for a flourishing trade in which merchants from India and the Persian Gulf took an active part. The use of special types of seals in different regions suggests the existence of merchant middlemen who maintained accounts, documented contracts and despatched sealed packages of goods. Their main centres were southern Mesopotamia, the Persian Gulf and the west coast of India, extending into the Indus valley on the one hand and as far as the Gulf of Cambay on the other.[4]

NOTES

[1]British Museum, Department of Western Asiatic Antiquities, seal number 128668.

[2]*Alisar*, xxxvii, Abb. 186.

[3]According to Dr. H.D. Sankalia, Ahar is a settlement of copper-workers, and the nearest source of copper is said to be 10 miles from Ahar. But it is yet to be ascertained whether these sources were worked as early as the beginning of the second millennium B.C. by the Ahar folk.

[4]I am indebted to Dr. André Perrot, Director of the Louvre, for permitting me to take photographs.

Part V Mesopotamian and Persian Gulf Maritime Trade with India

Editor's Introduction

Dilmun, Makan, and Meluhha are ancient place names often found in Mesopotamian economic and literary documents. Dilmun, or alternatively "Tilmun," is thought to have been the modern island of Bahrain, and the adjacent Arabian coast. The basis for this argument is given in the paper by Peter B. Cornwall reprinted here. More recent work by the Danish archaeological mission to the Persian Gulf has tended to confirm his conclusion (Bibby 1969). Samuel N. Kramer, however, arguing from the fact that several of the ancient literary documents state that Dilmun is in the land of the rising sun suggests that it must therefore be placed to the east of Sumer in either Iran or even the Indus Valley. Romila Thapar in a provocative paper (1975), the length of which precluded republication in this book, also suggests that Dilmun is to be identified with ancient India, as are Makan and Meluhha.

A. Leo Oppenheim's "The Seafaring Merchants of Ur" splendidly outlines much of what is known about the mercantile aspects of the Dilmun trade. Products traded, seasons of trade, and interesting insights into the organization of the Mesopotamian economy are all set forth in the paper. For those interested in pursuing this trade in greater depth two papers by Leemans (1960b and 1968b) are also essential.

The part is closed by S.R. Rao outlining possible indicators of contact between Susa and his site of Lothal and is a clear complement to his discussion of the Persian Gulf seal. The Leshnik paper titled "The Harappan 'Port' at Lothal: Another View" which will be found in Part VI should also be noted in connection with the maritime trade since it is a critical review of Rao's hypothesis.

The essential question to be asked of this body of literature revolves around the mechanism of trade (or exchange) between ancient India and Mesopotamia. The basis for knowing that there are objects, especially in Mesopotamia, which demonstrate some contact between the two civilizations has already been discussed. The next question is: How did they get there? The material from Tepe Yahya in southeastern Iran (Lamberg-Karlovsky 1971b, 1972a, 1972b; Kohl 1974) and Shahr-i-Sokhta in Seistan (Tosi 1968, 1969) clearly indicate that there was some degree of land contact with both the Indus Valley and Mesopotamia over the Iranian plateau. But the route in this case seems to have been a complex set of overlapping relationships. A maritime trade directly linking the civilizations and complementing the overland routes is another possibility, and the one considered here.

If the identification of Dilmun with Bahrain is provisionally accepted the location of Makan and Meluhha remains to be solved. The literary and economic documents of Mesopotamia indicate that the maritime trade from these two places was generally processed through Dilmun before it reached the cities of Sumer. But Sargon at about 2350 B.C. does boast that the ships of Meluhha, Makan and Dilmun were moored at his capital of Agade (Kramer 1964, reprinted in this part).

Another fact which has been noted in these documents (Oppenheim 1970: personal communication) is that when the countries are mentioned together the order is invariably Dilmun, Makan, Meluhha or Melukhkha, Makan, Dilmun. This strongly suggests a geographical order and since Dilmun is the clearing house for goods bound for Sumer it must be closest to Mesopotamia.

As noted by Oppenheim (1954) many products came to Sumer from Makan and Meluhha. The most important of these seems to have been copper, ap-

parently as ingots, and implements. Some of the references are to copper shipments which reach hundreds of pounds. Other materials—carnelian, ivory, shell, lapis, pearls (?), spices (?), etc.—were imported as well; however, the copper seemingly carried the trade.

Thinking for the moment about Meluhha, since it was the most distant and the one most frequently hypothesized to have been the Indus Valley, it should be remembered that the cuneiform documents state only that these products came from that country. From whence the Meluhhans obtained them is not noted, but a working assumption is that they probably came from places within, or not far removed from there. If this is accepted then an examination of the list of items coming to Sumer from Meluhha might prove useful.

The materials of direct relevance are: copper, carnelian, ivory, lapis, pearls (?) and shell. All of these, except pearls, have been found in archaeological contexts in Harappan sites. Carnelian bead-making seems even to have been an Harappan forté if the quantities of these ornaments from Indus sites is recalled along with the bead-making shops at Chanhu daro and Lothal. Similar quantities of shell bangles and inlay have been found, although no workshops have been outlined. Ivory was also widely used by the Harappans and complete elephant tusks have been recovered at Mohenjo daro, Lothal and Surkotada in Kutch. The copper is, however, another matter. There are no known deposits of ore in Sind, the Punjab, or Gujarat, seemingly the base, lowland centers of the civilization. Copper ore is found in some quantity, in Baluchistan and Rajasthan, both directly adjacent to the lowlands, and it is presumed that the rather abundant quantity of copper in Harappan sites was derived from these sources. A similar situation pertains to the distribution of other metallic ores and stone such as lapis and steatite. Pearls, if that is what the "fish eyes" actually were, would have been available from the Arabian Sea coast of Sind and Gujarat. Thus, there is nothing in this list of products which suggests that the Indus Civilization is an inappropriate place for us to hypothesize as Meluhha. It is true that other areas, eastern Iran and Seistan especially, come to mind as places consistent with these facts. But remember that Meluhhan boats apparently sailed to Sumer. Therefore Seistan and all except the coast of eastern Iran ought to be ruled out as alternative locations, and thus far the coast of eastern Iran has produced nothing in the archaeological record of the third millennium which even vaguely suggests a culture on the scale we must apparently associate with Meluhha. The Indus Valley is therefore the best alternative.

The case is still not proved. It remains simply a reasonable supposition or hypothesis to associate the Indus with Meluhha. The absence of Mesopotamian goods in Harappan contexts is clearly relevant here, and argues against the proposition. But an hypothesis such as this can be verified with carefully prepared research and renewed excavation, especially in Pakistan. Clearly, a task for the future.

The Seafaring Merchants of Ur

A. LEO OPPENHEIM

After many years of waiting, Assyriologists are now given the opportunity to study the numerous documents of the Larsa period excavated by C.L. Woolley in Ur (1922-1934).[1] With its more than 880 tablets, this volume is bound to become one of the most important publications for those few Assyriologists who strive for integration and a synthesis of the immense amount of socio-economic data of the Old-Babylonian period pouring forth every year from the cuneiform sources.

The present collection, which contains very little extraneous material, is offered by Dr. H.H. Figulla in copies which read well and is provided by him with an introduction of 80 pages containing: Descriptive Catalogue, Personal Names, Divine Names, Select Vocabulary, etc. The list of personal names is—in spite of minor shortcomings—indispensable for the understanding of the text material because the shortsighted and antiquated publication policy of the excavator deprives the reader of that essential source of information which becomes available only by studying, as a unit, the tablets found together in so-called caches, as "archives" of private persons, or in specific rooms of (public) buildings. Whoever intends to investigate, e.g., a specific aspect of the economic life of Ur, has to use the list of personal names to reconstruct these archives and to collect again the tablets scattered throughout this publication, which articulates the material—necessarily—according to the contents of the individual tablets.

As a publication of cuneiform texts, the present volume (UET V) deserves praise; much work has gone into the autographs and the compiling of the indexes. As to the contents, the letters are with a few exceptions not very interesting, the administrative documents would require a detailed study to yield all the potential information they hold, and the legal texts offer us a few new types of contracts apart from the bulk of standard texts which will, however, serve to establish the local peculiarities of the legal practices in Ur.

The most interesting and novel information contained in this body of Old-Babylonian texts has to do with the role of the town of Ur as the "port of entry" for copper into Mesopotamia during the time of the Dynasty of Larsa. The copper was imported by boat from Telmun,[2] today the island of Bahrein, in the Persian Gulf. This "Telmun-trade" was in the hands of a group of seafaring merchants—called *alik Telmun* —who worked hand in hand with enterprising capitalists in Ur to take garments to the island in order to buy large quantities of copper there. Since the island hardly yielded any ore—not to speak of the fuel needed for smelting—we are faced here with a situation which is typical for international trade on a primitive level: Telmun served as "market place," a neutral territory, in which the parties coming from various regions of the coastal area of the gulf exchange or sell the products of their countries. Seen from Southern Mesopotamia, Telmun and its "Hinterland" on the Arabian peninsula, formed the "doorway" to the East, to the more or less fabulous region of Makkan and Meluḫḫa through which certain raw-materials,[3] specific plants ("Kulturpflanzen") and breeds of animals came to Babylonia. The efficiency of Telmun in this role varied greatly from the days of Sargon of Agade to those of the Assyrian Sargonides and to Nabonidus according to the fluctuations of the political power of both Mesopotamia and that region of North-Eastern Arabia which most likely yielded the bulk of the raw materials. In our period—that of the fifth to seventh king of the Dynasty of Larsa—the island exported not only copper in

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ingots but also copper objects, beads of precious stones, and—most important of all—ivory.

Here is the evidence: Travels to Telmun are repeatedly mentioned in a group of tablets which come patently from the archives of the temple of the goddess Ningal and list votive offerings, incoming tithe, etc. The contexts suggest that returning sailors were wont to offer the deity in gratitude a share of their goods. In UET V 526 we read of a small amount of gold, copper, and copper utensils characterized as "tithe of the goddess Ningal from an expedition to Telmun and (from) single persons having gone (there) on their own," during the first 3 months of the year. UET V 292 has a similar "subscription" "from expedition(s) to Telmun by . . . -boat and (from persons) having gone (there) on their own," but its listing of merchandise is more extensive; besides "red" gold, copper, lapiz lazuli in lumps, various stone beads, ivory-inlaid tables, etc., we find also "fish-eyes"—perhaps pearls.[4] The parallel text UET V 286 enumerates more luxury goods and so does UET 678, recording ivory combs, eye-paint and certain kinds of wood, not to mention designations which we fail to understand.

From the same archive come also texts—such as UET V 280, 285, 286—which indicate that the presenting of such precious objects, beads, etc. was voluntary, as an expression of thanks for divine protection. That the *alik Telmun* needs such protection is shown by the unique blessing in a letter addressed to a sailor: "May Ea and the god Amurru keep you alive through many days for my sake" (UET V 71). Indeed, the lú. má(!). gal. gal (UET V 279:7) Awēl-Sin offers to Ningal not only silver rings and kidney-shaped beads of red stone but also "fish-eyes" and a pectoral of ivory, both, we may well assume, acquired in Telmun.[5]

The exact nature of the business transactions typically performed in Telmun is unequivocally stated in UET V 367: "2 mina of silver (the value of): 5 gur of oil (and of) 30 garments for an expedition to Telmun to buy (there) copper, (as the) capital for a partnership, L. and N. have borrowed from U. After safe termination of the voyage, he (the creditor) will not recognize commercial losses (encurred by the debtor); they (the debtors) have agreed to satisfy U. (the creditor) with 4 mina of copper for each shekel of silver as a just [price (?)]."

The crucial clause of this contract shows the creditor refusing expressly to share the possible losses of the enterprise. The same proviso recurs in UET V 415:11-12 and in 426:24-25; it has to be linked to a passage of the series *ana ittišu* (Landsberger 1937:36, Tabl. III i:60). There our clauses establish the meaning of d a g. gi_4.a (Akk.:*babtum*) by showing *ibissû* "losses" in exactly the same context; *babtum* must therefore denote some kind of customs or dues imposed upon the merchants by the city administration.

The wording of this new clause is important since all extant Old and Neo-Babylonian contracts on partnership reserve for the *tamkarum* not only the invested capital (plus interest) but also an equal share of the profit yielded by the business venture. In our instance, however, the capitalist has refused to share the possible losses (in oversea ventures: UET V 367 and 428) or other expenditures (UET V 415) of his traveling partners and has to content himself therefore with a fixed return for his investment, instead of sharing the entire profit. It should be stressed in this context that the Code of Hammurabi (§"98") attempted, unsuccessfully, to compel the investing merchant to share not only the profits but also the losses of the traveling merchant (Eilers 1931:36f.).

It seems that the disadvantageous position of the entrepreneur in Ur was due to two reasons: the extremely hazardous circumstances typical for the mercantile venture which is in the nature of this transaction, and the necessity to utilize a means of transportation which is expensive and requires specialized personnel.

Oversea trade with its enormous returns—in spite of the *risicum maris et gentium*,[6] as mediaeval law puts it, places the merchant traveling by boat in a very favorable position compared to that of his colleague who follows the caravan routes. Within Mesopotamia the latter seems to have been primarily concerned with the distribution of certain staples, the retailing of cheap manufactured goods as well as with the interurban exchange of merchandise, while the Telmun trader imported to the country not only an essential raw material, copper but also highly priced luxury goods which were easily marketable in the emporium. More important to the enhancement of the position of the *alik Telmun* was the fact what while the traveling overland merchant was most likely beset by competition and, consequently, short of investors, the Telmun trade seems to have been in the hands of relatively few persons due to the technical skill the oversea voyage required and probably also because of the necessity to have personal contact (accreditation) on the island. The *alik Telmun* with their Telmun-boats (i.e. "Telmun-going") certainly did everything to pre-

serve this state of affairs which was so much to their advantage.

The investing merchant, quite naturally, attempted to improve his position. This tendency is expressed in the text UET V 297, a unique example for a creditor borrowing money from himself: "11 shekels of silver, as a *qiptu*-loan (i.e. a loan without interest), Paṣṣê, Imgur-Sin, Ahušunu, Ilišu-nāṣir and Zubābum have borrowed from Zubābum. On the 30th of Simānum, any of them who is (physically) well [and solvent will repay the silver to Zubābum])." For the simple reason that he could expect in this way a larger return for his investment, the creditor changes here into a full-fledged partner who is entitled to share without limitation in whatever profits the enterprise would yield.

This tendency of the investing merchant happens to tally with the desire of the traveling merchant to spread the risk of the venture. The division of the risk represents in fact a very primitive form of maritime insurance which precedes that in which the risk is transferred—for a fee—to other persons under various forms, i.e. real marine insurance. Examples for the distribution of the risk in maritime ventures are not too frequent in the texts from Ur. Cf. UET V 391, where three partners borrow barley and silver from a woman with a clause (lines 11-15) which reveals the nature of the business "they (the debtors) will pay the hired persons (i.e. crew) and (the rent of) the boat[s] and M. and I. (two of the 3 partners) will divide (the profit) in equal parts." Note especially UET V 192 recording the sale of a share of a maritime venture "a share of the maritime expedition to [ ] belonging to PN_1 Sin-māgir has bought from PN_2 and PN_3 (the parents of the owner)." Attention should in this context be drawn to the fact that a rather large number of partnership contracts can be found in the present collection, such as UET V 126 (2 partners taking an investment loan for 5 years), 130 (3 partners), 362 (2 partners), 415 (2 partners to buy barley), 417 (2 partners).[7]

The complex legal relationship between the investing and the traveling merchant has created a number of loan types of which at least two are mentioned in the Code of Hammurabi. One of them uses the characteristic term *tadmiqtu*. We encounter this word in the paragraphs 102-103 of the Code and in a few documents of that period. It is therefore rather important to find in our publication (UET V 428) a reference to this technical term.

The interesting tablet UET V 428 seems to contain two sample texts for loans connected with the oversea trade of Ur. The first of these formulae (lines 1-9) runs as follows: "5 shekels of silver as a tadmiqtu-loan PN_1 has borrowed from PN_2. He will return the silver at a moment (yet) to be determined (?) (This) he has sworn by the life of the king."

The specific designation of the loans as *tadmiqtu* "favor, kindness" (in Sumerian: KA. sa_6 "friendly word") should not, in spite of the obvious etymology of these terms in both languages, induce us to presume that this business transaction was not as completely under the sway of the laws of economic life as any other loan. Still, the etymology must be taken to reflect a change in the creditor-debtor relationship which naturally occurred under economic pressure and replaced, perhaps, in credit operations, security by that intangible quality of the borrower which we call credit.

Normally, loans lent by investing capitalists are due at the safe return of the expedition.[8] The pertinent phrase occurs in three of our texts (UET V 313, 314 and 315) while all hitherto published Old-Babylonian legal documents (nearly 2000) offer us only five more instances.

As to the main object of the Telmun trade, the copper (termed URUDU), we obtain most of the evidence from the letters (UET V 22, 29, 71 and 81) addressed to a certain Ea-nāṣir, a traveling merchant and importer of Telmun copper. The metal came in large quantities (UET V 796 mentions more than 13,000 minaz of copper according to the weight standard of Telmun) and often in ingots termed *gubarum* which weighed up to 4 talent each (UET V 678). The ingots are sometimes qualified as *damqu* (UET V 22, 81) as is also the copper itself (UET V 20 *wariam la damqam*, but *wariam dummuqam* in UET V 5 and 6). The quoted passages do not entitle us to speak of refining of copper, because Ea-nāṣir was not a coppersmith but a merchant and because the meaning of *damqum* as well as of *dummuqum* as "good (in quality)" is borne out by such letter passages as UET V 5:28 or 22:10-13 ("show him 15 ingots so that he may select 6 *damqu* ingots" cf. for the selecting of such "good" ingots also UET V 81:50-51).

Especially revealing with regard to the background of the Telmun trade is the long letter UET V 81. The writer complains bitterly that our Ea-nāṣir had promised to give to his messenger only good copper ingots while he, in fact, showed him bad ones with the added insult of a remark which is the Old-Babylonian equivalent to our "take it or leave it." Outraged, the

writer of this letter exclaims: "Who am I that you are treating me in this manner and offend me? (that this could happen between) gentlemen as we (both) are!" And he continues, "Who is there amongst the Telmun traders who has (ever) acted against me in this way?" More important, however, is the argument of Ea-nāṣir which the writer quotes verbatim in lines 33-39: "I myself gave on account of you 19 talents of copper to the palace and Šumi-abum gave (likewise) 18 talents of copper, apart from the sealed document which we both handed over to the temple of Shamash."

From this we learn that the palace levied considerable custom duties upon the imported copper. The texts UET V 667, referring to copper transactions of the palace, and 558, mentioning (in broken context) taxes on importing boats seem to illustrate this very situation.

Not only the raw material but also copper utensils have been brought over from the island. Ea-nāṣir, e.g., is supposed to have imported a large copper kettle (UET V 5:25).

The economic importance of that segment of the trade which does not concern itself with metal and metal objects is brought to the fore by the fact that the second of the two formulae contained in the "sample"-text UET V 428 refers to such activities. The text is rather difficult but the following translation might be attempted: "1 mina of . . . silver, 1/2 mina of . . . silver to buy (precious) stones, "fish-eyes" and other merchandise on an expedition to Telmun, PN_2 has borrowed from PN_1. After safe completion of the journey, he (PN_2, the debtor) will bring the equivalent of the silver and will (thus) satisfy the capitalist; the capitalist (however) will not recognize . . . (This) he has sworn by the life of the king." Whatever the reading and the meaning of the three last signs of line 24 be, the document reflects the already observed atypical relationship between creditor (investor) and borrower (traveling agent) which characterizes the situation in the Telmun trade: the returns of the former are restricted to a fixed amount while the latter is to enjoy all the profits of the enterprise. In our specific case, a clause has been added which seems to protect the investor in some respect.

An unusually fascinating problem faces us with regard to the fact that ivory as raw material (UET V 546) as well as finished ivory objects have been imported from Telmun. Among the latter we find exactly the same objects which we know so well from the dowry inventories, etc. of the Amarna letters: ivory combs (UET V 292, 678), breast plates (UET V 279), boxes (UET V 795), inlaid pieces of furniture (UET 292) and spoons (UET V 795).

If one compares the ivory objects of the Larsa period with those mentioned in the volume Legrain UET III dating to the time of the Third Dynasty of Ur one notes a decided difference: ivory was used here principally for small figurines (human and animal shaped) and for small round objects such as "apples" or end-pieces for the legs of chairs (claws or hoofs), etc. This again is in harmony with evidence at hand concerning the very few extant ivory objects found in early Babylonia as to their size and nature. Apparently, two "styles" of ivory work should be recognized in this period, one originating in the native use of imported material and the other—represented so far only in written documents—to be found in imported ivory objects.[9]

One more observation in this respect: no ivory objects are, to my knowledge, mentioned in the economic and administrative texts of the Old-Babylonian period (later than those of UET V) nor, as a matter of fact, in those of the Middle- and Neo-Babylonian. Even the royal inscriptions of the Chaldean kings, with their glowing reports on the sumptuous decorations of the palaces and sanctuaries, mention ivory only very rarely and in a way that shows that it was available only in small pieces. It stands to reason that Southern Mesopotamia remained outside of that large area in which—during the second half of the 2nd and the first of the 1st millennium B.C.—the use of ivory spread from Egypt and Syria.

In view of the above outlined distribution-pattern of the references to ivory, those of our publication (UET V) assume added importance because they show that Southern Mesopotamia had to rely exclusively upon ivory imported from the East, to be exact: via Telmun. The interruption of this trade route after the fall of the Dynasty of Larsa (cf. below) put a sudden end to the use of ivory in our region.

Since Telmun was only a market place, two possibilities have to be envisaged: the ivory obtained there by the traders of Ur could have come either from Egypt (through some unknown commercial channels) or from India brought across the Indian Ocean on boats sailing with the monsoon. In favor of the second alternative speak the well-established links between Southern Mesopotamia—especially Ur itself—and the civilization of the Indus valley. The discovery of Indian seals (Gadd 1932) and of specially treated carneol beads (Frankfort 1924/27:138ff.) in Mesopotamian

excavations has proven beyond any doubt the existence of such trade relations. We now may well add ivory to this list as an item based exclusively in Mesopotamian sources on philological evidence, while we have from Mohenjo-Daro actual ivory combs.[10]

With regard to economic role and business practices, a comparison between the merchants of Ur engaging in the Telmun trade and the Assyrian traders of Anatolia of about the same period suggests itself as a means to establish more clearly the deep-seated differences as against the rather superficial similarities. To speak first of the latter: in two passages of the Ur documents the *karum* is mentioned in a way which reminds one of the function of this important institution in Asia Minor. UET V 82 refers to the *karum* as a locality in which business accounts have been settled, which in Old-Babylonian practice is normally done in the temple of Shamash: "the very day I decided to leave for the (overland) expedition, we sat down in the *karum* upon the request of Šamaš-ellassu and settled our accounts in the presence of Ili-tûram." UET V 114, a legal document, has the phrase "the estate (literally: the house) of Awiātum, whatever there is within the city or in the *karum*" which indicates that (*a*) the *karum* was outside of the city proper and (*b*) that assets of the capitalist could be maintained and managed either from within the city (as property of a free citizen) or from the *karum* of which the person was a member in regard to his relationship to inter-city business transactions. It seems furthermore worth while to point out here the above quoted passage from the letter UET 81 in which one merchant reminds the other of the obligation of being a *mâr awēlim* i.e. to adhere to certain ethical and social standards in business transactions. Very similar phrases can be found in the correspondence of the Old-Assyrian merchants, "act according to your status as gentleman!" and *lā-awēlum* "un-gentleman." The importance of textiles as an ideal export item—light in weight, cheaply produced by slave labor from abundant and inexhaustible stores and always welcomed by the "barbarians"—offers another link in our comparison. The Ur text UET V 848 shows that our Ea-nāṣir received 50 garments obviously destined to be brought to Telmun in exchange for copper etc. exactly as the 30 garments (UET V 367) which a certain Lu-Meslamtaea (and his partner) received for the same purpose as this text states expressly.

The differences in the backgrounds of the Telmun trade and of the Anatolian merchant colonies are, however, very essential. The former acted within the framework of a central government and was accordingly subject to taxes and duties (without, most likely, enjoying any military or political protection) while the latter formed a close-knit independent organization within a foreign country where the merchants had to rely upon themselves and their ability to cooperate with a variety of city rulers and the ever changing political situation. It is, of course, very likely that these specific circumstances favored or even caused a development of the *karum* institution in Asia Minor beyond its original function and limitations in the Mesopotamian world. The absence of any mercantile activity or aspiration among the natives of Anatolia created the unique setting of the activities as to the mobility and independence of the Assyrian traders. Equally unique seems to have been the position of the island of Telmun as a link between the various coastal regions of the Persian Gulf with its outlet to Southern Mesopotamia monopolized by the sea-faring merchants of Ur.

From the point of view of commercial techniques—legal practices, business terminology, etc. —the Assyrian traders seem to be much more advanced than their Babylonian colleagues although the latter's activities are so far attested only in a small group of documents as against the many thousands of so-called "Cappadocian" texts.

The Telmun trade flourishing in the Ur of the time of the Dynasty of Larsa is, of course, not an ephemeral phenomenon. Texts from the very same city, written during the rule of the last king of the Third Dynasty of Ur reflect the existence of similar trade relations with the East although the economic situation was then essentially different.

In the large collection of Ur III tablets excavated in Ur (Legrain UET III) we find a small number of documents which assume significance in the light of the investigation undertaken in this article. Fortunately, these few tablets happen to contain rather important and unequivocal information. A certain Lú-^{d}En-lil-lá is said in UET III 1689 (Ibbî-Sin, 4th year) to have received large amounts of garments and wool from the storehouse of the temple of Nanna in order to buy copper in Makkan (n í g. š á m. m a u r u d u M á. g a n ^{ki}, literally: equivalent for buying copper in M.). The same individual receives again wool, garments, (perfumed) oil, and certain leather objects (text damaged) which are destined—as the text (UET III 1511, Ibbî-Sin 2nd year) states—to buy copper. The last line of

this tablet indicates furthermore that these goods were loaded on a boat bound for the same country—Makkan. These two tablets (cf. also the similar but broken UET III 1666) have to be connected with the text UET III 751 (Ibbî-Sin 2nd year) which lists materials which our Lú-^{d}En-líl-lá delivers to the temple of Nanna as tithe. And these materials are not only copper, as we expect, but also beads of precious stones, ivory, and onions. The latter are described expressely as Makkan-onions.

Obviously the commercial activities of Lú-^{d}En-líl-lá are exactly of the same type and nature as those of Ea-nāṣir who lived in Ur during the time of the Dynasty of Larsa. But while Ea-nāṣir exported garments, oil etc. to the island of Telmun to bring back from there copper, beads, and ivory, Lú-^{d}En-líl-lá who lived under Ibbî-Sin, last king of the Third Dynasty of Ur, went to Makkan instead of to Telmun to import and export the very same goods. The analogies of these trade relations is further underlined when one remembers that the pre-Sargonic Sumerian texts show a kind of onion which they call Telmun-onion (cf. simply Deimel 1925:24).

There are, however, two rather interesting points of difference.

While Ea-nāṣir is referred to rather concisely as *alik Telmun* (cf. above), his precursor Lú-^{d}En-líl-lá is given the title ga.eš$_x$.a.ab.ba on his seal of which we have an imprint on the tablet UET III 41 (Ibbî-Sin, 18th year). This old-fashioned designation of a seafaring merchant is attested already in pre-Sargonic texts and in those of Ur III which come from the capital of the realm. The older texts speak even of a ga.eš$_x$ maḫ as a functionary of the palace. It is important in this context to point out that the activities of the ga.eš$_x$ were typically connected with travels to Makkan. This is shown by an Ur III (?) tablet and by references to a historic event mentioned by Ur-Nammu, first king of the Third Dynasty, on a clay cone found in Ur (UET I 50) as well as in the preamble to his law-code (Kramer 1952: Fig. 13). The text from Ur, translated here after Th. Jacobsen has Ur-Nammu describe himself as the one who "restored trading along (?) the coast and in (a locality which he calls) ki.mú and made return the Makkan-boats to him (his god Nanna)." Other texts show the ga.eš$_x$ dealing with commodities such as wood, perfumes, precious stones (UET III 341) which, later on, are the concern of the *tamkarum*.

A second and more important difference concerns the economic background. The position of the trader, the seafaring merchant, seems to be rather similar in both instances: Our man noted above as well as Ea-nāṣir sail with their boats to Makkan or Telmun, respectively, to exchange their cargoes of Mesopotamian staples against the products of these regions. However, while the ga.eš$_x$.a.ab.ba received his stock of trade from the temple, in fact, from the officials in charge of the various specialized storehouses of the temple of Nanna in Ur, the *alik Telmun* was provided with funds and merchandise by private persons, investing capitalists. This can well be taken to reflect that crucial change in the economic and social structure of Mesopotamia which occurred between the Third Dynasty of Ur and the Dynasty of Larsa. This evolution seems to have given rise to a situation which favored the accumulation of wealth in the hands of private individuals who, as our texts illustrate, invested it in oversea trade ventures which ensured the best yield in spite of their innate risks.

For those interested in economic history, these changes in the mechanics of international trade may be rather interesting and informative. Equally relevant can be a comparison of the Mesopotamian techniques with their characteristic dichotomy: investing party (be it the temple organization or private citizens) and seafaring party (always in private hands) with those which we find exemplified in the expeditions of Egyptian kings (to Punt-Pyene) or temples (Wen-Amun) and the commercial ventures of King Solomon (Ophir).

The following picture of the history of the Eastern trade of Southern Mesopotamia can be deduced from the extant evidence. A process of gradual and slow restriction of the geographical horizon marks the entire development of these commercial connections. We may well assume that the frequency and intensity of contact had reached a peak early in the third millennium B.C. When Sargon of Agade proudly proclaims (Legrain 1923:208 f., col. v-vi) that ships from or destined for Meluḫḫa, Makkan, and Telmun were moored in the harbor which was situated outside of his capital, this obviously proves the existence of flourishing commercial relations with the East. The question, however, remains unanswered as to whether this trade was in the hands of the foreigners or the natives. The Semitic Akkadians make the former alternative seem more likely. If this is correct, the active participation of the seafaring merchants of the Ur of the Third Dynasty as well as that of the Dynasty of Larsa could well be considered the second stage of a

development which reflects the diminishing power of expansion of the East. The Mesopotamians would only then have traveled to Makkan and Meluḫḫa when the boats of the latter ceased coming to their ports. In the period documented by written sources, Meluḫḫa is already outside the borderline of actual contact with Mesopotamia—in spite of references to it in the so-called "Geographical Treatise."[11] Neither wars nor tribute are mentioned and no names of kings or private persons of Meluḫḫa are known. This Ultima Thule is only said to be the homeland of certain raw materials (copper, stone, timber) or the native habitat of a few plants and breeds of animals.[12] This is not the case with Makkan and Telmun. Both countries continue to be mentioned in economic and other texts through the entire period of the rule of the Third Dynasty of Ur. In the documents excavated in Ur, Makkan is a definite geographical and economic reality during that period. We even know the name of a person, a native of "Great-Makkan" i.e. Ur-Nammu (UET III 1193). In this period, Makkan—"the country of mines" seems to have been the only importer of copper. References to Telmun are rather rare in UET III. Apart from UET III 672, which is without value, we have only UET III 1507 mentioning a skipper who receives, quite true to type, wool and garments to load upon a boat going to Telmun.

After the collapse of the Dynasty of Ur, Telmun replaces Makkan in the Eastern trade of the city. The latter country is not mentioned any more in our volume of tablets of the Larsa Period. Telmun, as against Makkan, seems never to have completely lost contact with Mesopotamia. The references, however, to such incidents are extremely rare in certain periods. The rising First Dynasty of Babylon seems to have had no political or commercial relations with Telmun but the letters from the archives of the palace in Mari, as far as published, do contain references: a messenger from Telmun who was detained in Mari on his way to the king on account of an incident which occurred in the house of a *tamkarum*, and a letter of Iasmaḫ-Adad to Hammurabi of Babylon concerning a caravan sent by the king of Mari to Telmun which was detained and which Iasmaḫ-Adad asked Hammurabi to keep in Babylon until further order. One is under the impression that the commercial relations with Telmun have been within the range of normal activities of this type although no indication as to the nature of the staples imported and exported can be found. Two letters from Kassite Nippur (time of Burnaburiaš), transliterated and translated by A. Goetze (1952), show the island much more as a supplier of certain kinds of dates than as a maritime emporium. Obviously, Telmun had lost contact with the mining centers of Makkan and with those regions which supplied it with stone and timber, etc. some time between the fall of the Dynasty of Larsa and the decline of power of the Hammurabi Dynasty. It turned again into an island famous only for its agricultural products,[13] its sweet water, etc.[14] Copper, precious stones, and rare woods had now to come to Southern Mesopotamia either over the mountain ranges and from the West along the river routes. The toponyms Makkan and Meluḫḫa have then been, as is well known, transferred to two other far-off countries situated in the southern limits of the geographical horizon. If this transfer was prompted—as Landsberger (1924:217, n. 2) suggested already—by the fact that the same raw materials came now from the "Southern" Makkan and Meluḫḫa which formerly had been supplied by the eastern regions of these names, our evidence concerning Eastern ivory bought in Telmun adds another point in favor of this argument.

As to the political history of Telmun, it should be stressed here that there exists no evidence whatsoever that the island was under the rule of a Mesopotamian king up to Burnaburiaš (Kramer 1944). The claims based upon the "Geographical Treatise," the Sargon-Legend, etc. and the text of the Statue D of Gudea cannot prove Mesopotamian supremacy over Telmun (as against Cornwall 1952).

Sometime in the second half of the 2nd millennium B.C., Telmun seems to have come in closer contact with the rulers of Southern Babylonia (Goetze 1952). When Telmun appears beside Meluḫḫa among the titles of Tukulti-Ninurta I, one has only to consider the context of that statement ("king of Sippar and Babylon, king of Telmun and the country Meluḫḫi, king of the Upper and Lower Seas, king of (all) mountain-(regions) and of the deserts") to see that this means, at best, that he was aware of the fact that Telmun "belonged" to the realm of Babylonia, but the reference could also be an antiquarian reminiscence like Esarhaddon's hymnical boast "[king] of the kings of Telmun, Makkan, Meluḫḫa" at the end of his titulary.

With Sargon II, however, we begin to tread upon more solid ground. The fact that Uperi, king of Telmun, sent tribute to the Assyrian king after the latter defeated Merodach-Baladan is obviously a diplomatic step meant to appease the new power in

Southern Mesopotamia. Even the sending of a detachment of soldiers with appropriate tools to help Sennacherib in razing the conquered city of Babylon can be interpreted as a rather unusual demonstration of diplomatic "friendship" but does not necessarily imply Assyrian rule over Telmun.

As already foreshadowed by the incorporation of Telmun in the "list of provinces," the island entered the full limelight of Assyrian foreign policy with the offensive of Assurbanipal against Southern Mesopotamia, Bît-Iakin, and Elam. We are fortunate indeed to have three letters at our disposal, two written by Assurbanipal's general Bêl-ibnî mentioning Ḫundaru, king of Telmun, and one written by Assurbanipal and addressed to Ḫundaru. The details of the dealings of the king of Telmun in his fight for survival are of little interest in the present context, far more revealing is the mention of metal (bronze), precious woods and "*kohl*" i.e. eye-paint in these letters. We read of great amounts of kohl, 26 talent of bronze, numerous copper and bronze objects, of sticks of precious wood as part of the booty taken from Telmun, while another speaks of the tribute of Telmun mentioning, at the same time, bronze, perfumes and likewise "sticks" of precious wood offered by merchants from Bît-Naịalu. All this has to be connected with a passage of the inscription KAH 122 of Sennacherib (quoted above) which describes the tools of the crew of corvee-workers sent from Telmun to Babylon to assist the Assyrian king to tear down the city. Their tools are characterized as follows: "bronze spades and bronze pikes, tools which are the (characteristic) product of their (native) country." Thus, it becomes evident that Telmun has again access to the copper mines of Makkan, to the spices, perfumes and rare woods of the East. The merchants are importing again these staples into Mesopotamia: the mysterious barrier which interrupted the trade routes towards the East for a full millennium seems to have been removed. What movements of nations, changes of military and political power or technological developments had been at work to first restrict the Eastern horizon of Southern Mesopotamia and to open it again—we will probably never know.

But the Persian Gulf is again accessible for the West. Assurbanipal's inscription in the temple of Ishtar in Niniveh reflects this change in its listing of the rulers of that region who bring their tribute to the king. New names and new—as well as old—countries appear here. Assurbanipal stresses that no messenger of Kuppi or Qadê where king Padê ruled in the town of Iskê has ever before set foot on Assyrian soil, they had to travel six months to bring their gifts and to greet the king. Another island—beyond Telmun—is then mentioned: "[*x-y*]-*i-lum*, king of the [ ]-people who resides in Ḫazmani which is an island alongside Telmun" whose messengers had to travel a long way across the sea and overland to Assyria.

Since only two centuries later, the Achaemenian kings of Susa wore magnificent necklaces of real pearls it stands to reason that Indian pearl-fishers must have moved into the Persian Gulf rather soon after the change just described took place in the political situation. The pearls of the Gulf and of Telmun which then began to penetrate towards the West, have been swept through the entire Near East by the tide of international trade released by the world-shaking expedition of Alexander. This import of the East was distributed by the same merchants who formerly plied the sea with boats loaded with copper, stone beads, and perfumes. "The merchant seeking goodly pearls" became a familiar figure and a symbol of the now open door to the East which was not to be barred again.

NOTES

[1]Editor's note: This paper was originally written as a review article for H.H. Figulla and W.J. Martin's *Letters and Documents of the Old Babylonian Period*, Ur Excavations, Texts Volume V, Philadelphia and London, 1953. This publication will hereafter be abbreviated "UET V." Other Ur Excavation volumes will be similarly treated. Since many of the points raised by Professor Oppenheim relate to linguistic arguments and presentation, some of the citations and footnotes have been edited from his text. Citations relating to economics and trade have, however, been retained.

[2]For literature on Tilmun (or Dilmun) see Cornwall 1944, 1946, 1952. Tilmun is associated with the famous red stone, of which Gudea speaks repeatedly as being imported from Meluḫḫa.

[3]See the conveniently arranged evidence collected by Weidner (1952/53:7, 9).

[4]The meaning "pearl" for IGI. ḪA has been proposed by R.C. Thompson (1936:53, n2) on the basis of UET V. Real pearls are extremely rare in the pre-Greek ancient Near East; although the exploits of Gilgamesh cannot fail to remind one of the techniques of the pearl-fishers. The appearance of rather numerous references to IGI. ḪA in Ur and especially in connection with imports from Tilmun must be considered an argument in favor of an interpretation which is not based on philological evidence. The lack of archaeological proof for the use of pearls is of course an important argument against the identification but its value is somewhat

diminished when one considers that no ivory object has been found in Ur although the texts report on ivory as raw material as well as on ivory objects.

[5]Another way to show the sailor's gratitude to Ningal—this goddess was apparently considered the "Notre Dame de la Garde" of Ur—consisted in the dedication of silver models of the sea-going boats. A human sidelight: in many instances women appear in the temple records which list offerings of beads and small pieces of jewelry, but they appear side by side with the *tamkaru*'s, the investing merchants, so that it cannot be argued that they are mentioned because the sanctuaries involved belonged to goddesses (Ningal and Bau). It seems more likely that the wives of the crew and the investors worrying about the cargo turned both to these goddesses and fulfilled their vows. With regard to the mention of boats, it might be worth while to point out that they are far more frequent in our volume (UET V) than in that of Legrain (UET III, time of the Third Dynasty of Ur) although here they are exceedingly large: 3000 gur in UET III 272 rev vi as against a maximum of 40 gur in UET V 229. In UET V 109, 119 and 270 boats are even used for fishing. I would like to interpret this situation as reflecting a larger share of oversea traveling (and deep-sea fishing) in the Larsa period of Ur than in that of the time when the city was the capital of an empire. This again dovetails with another observation: references to inland navigation abound in UET III but are rather rare in UET V. Apparently, foreign trade was either by-passing Ur (concentrated perhaps in its harbor area) or not in the hands of the natives while inland navigation was of great importance during the empire period; when, however, the city of Larsa became the capital, and the political and economic power of Ur was on the wane, foreign trade seems to have constituted its main source of income.

[6]References to activities of an enemy are found in UET V 20.6-11 ("since x years I have entr[usted to you] silver but on account of the enemy you offer me only bad copper!"), UET V 81:23-25 ("repeatedly you have made them (i.e. my messengers) return empty handed through enemy territory!) and 42-45 ("you have held back my invested capital in enemy territory (although) you are obliged to hand it over to me in full").

[7]The formation of partnership-relations under the above described condition should not be confused with the very practical banding together of travelling merchants for mutual protection against the dangers of overland travels. This is attested by the word *ellatu* (literally: "pack (of dogs)") for caravan in the so-called "Cappadocian texts" (Landsberger 1925.233) See also Albright (1942:36 and note 80), which denotes a flotilla of merchant men sailing together for mutual protection.

[8]To protect the ship and fittings, UET V 230:20-25 states: "the well-preserved ship and its fittings he will (retu)rn to its owner in the harbor of Ur intact (?)."

[9]Only an Old Babylonian text from Susa DP XXIII 310:9 mentions ivory combs.

[10]See Mackay (1948:pl. 24, no. 1). One further remark concerning the relations between Southern Babylonia and India may be permitted in this context. The text UET V 295:rev. 11 mentions a monkey made of red stone, beside a frog charm of the same material. A small monkey made of alabaster was found in the Ishtar-Kiyitum-temple in Ishchali by the Oriental Institute under H. Frankfort. Editor's note: Professor Oppenheim has an interesting, but detailed, discussion of the linguistic status of the term for monkey which has been deleted here. He does, however, end the discussion with: If the proposed reading of the Sumerian term should prove correct, the old crux of the etymology of Akkadian *uqupu*, Hebrew *qup*, Greek *kepos*, Egyptian *q'p* and Sanskrit *kapi* will be further complicated.

[11]See *Keilschrifttexte aus Assur Religiosen Inhalts*, No. 92 and Weidner (1952,53 1ff.).

[12]For wood described as coming from Meluḫḫa see UET III, 818, 1241, 1498 iv and rev i For copper see UET III 368 An ivory object representing a bird called DAR^{musen} Me luḫ. ḫa is repeatedly mentioned (UET III 757, 761, 764, 768, 770).

[13]Telmun dates are mentioned only once in UET V: see 590, 4.

[14]For mythological and literary connotations of Sumerian Dilmun see Kramer (1944).

On the Location of Dilmun

PETER B. CORNWALL

In his article entitled "Dilmun, the Land of the Living," which was published in the December 1944 issue of the *Bulletin of the American Schools of Oriental Research*, Dr. S.N. Kramer contended that "in all probability it is in southwestern Iran . . . where Dilmun is to be sought, and that it is not to be identified with the island of Baḥrein." I hope to show, on the contrary, that no longer can there be any reasonable doubt that Dilmun was the island of Baḥrein—and also, at least during some periods, a stretch of territory on the nearby Arabian mainland.[1]

But it is essential to recognize at once that the cuneiform references to Dilmun fall into two distinct categories: (1) historical, commercial, epistolary, dedicatory, and astrological inscriptions; (2) Sumerian literary compositions. In the former, Dilmun is a definite geographical locality—of that we may be certain. In the latter it is a fabulous land, a strange antechamber to the spirit world. That the two Dilmuns were thought of as being somehow identical is very likely[2]; but I suggest that Dr. Kramer should not have decided the location of Dilmun merely on the basis of Sumerian literary compositions, especially since the evidence gleaned from the other class of source material points decidedly to the equation Dilmun = Baḥrein.

The most important evidence directly supporting the latter conclusion is the following.

(1) Some inscriptions (Luckenbill 1926: Vol. 2, sect. 41, 70, 92, and 185; Cornwall 1944: Vol. 2, sect. 81 and 99) of Sargon of Assyria state that Upêri, King of Dilmun, "lives a fish 30 *bêru* away *in the midst of the sea of the rising sun*."[3] And one inscription from the time of Asshurbanapal declares that Dilmun "is in the midst of the Lower Sea" (Luckenbill 1926: Vol. 2, sect. 970).

These references imply that Dilmun was an island in the Persian Gulf. The Assyrian expression "in the midst of the sea" (*ina qabal tam-tim*) is a frequent term for an island: it is used, for example, to describe Tyre (Luckenbill 1926: Vol. 2, sect. 779 and 970), Arvad (Luckenbill 1926: Vol. 2, sect. 780, 783 and 848), and Cyprus (Luckenbill 1926: Vol. 2, sect. 309 and 326) in "the Upper Sea," or Mediterranean. Indeed, in the above mentioned inscription of Asshurbanapal a direct parallel is drawn between Tyre and Dilmun, for the Assyrian monarch boasts that he "established the yoke of his rule over Tyre, which is in the midst of the Upper Sea, and Dilmun, which is in the midst of the Lower Sea" (Luckenbill 1926: Vol. 2, sect. 970).

(2) The distance to the island of Dilmun is given as 30 *bêru*. Dr. Albright, pointing out clearly that this reckoning must refer to the number of hours required to reach Dilmun by sea from the starting point, continues: "It would be a very slow bark that could not make 5 miles an hour or 10 miles a *bêru*. Even at this modest speed 30 *bêrê* would be 300 miles, nearly the distance from Baḥrein to the mouth of the Euphrates in Sargon's reign" (Albright 1919:183).

It should be added that the prevailing winds in the Gulf are from the northwest. Having travelled on the Gulf in Arab boats, I am of opinion that a trip *from* the Euphrates to Baḥrein could have been made quite easily in "30 double-hours." And Arrian, it is worth noting, states that Tylos (the classical name for Baḥrein) "was said to be distant from the mouth of the Euphrates about a day and a night's voyage for a ship running before the breeze."[4] So the Assyrian estimate appears to suit Dilmun = Baḥrein with no strain whatsoever.

(3) Etymology strongly favours the contention

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that the name *Tylos* was derived from *T/Dilmun.*[5]

(4) A cuneiform inscription[6] found on Baḥrein in 1879 reads: "Palace (of) Rimum, servant of (the god) Inzak, man (of the tribe of) Agarum."

The lapidary script is archaistic Old Babylonian with forms (especially the *LÚ*) pointing to the second half of the second millennium B.C., and there can be no reasonable doubt about the translation. *Inzak* is known from another source[7] to be the name under which the Babylonian god Nabu was worshipped on Dilmun. It is surely of high significance that the only cuneiform inscription so far discovered on Baḥrein should mention the local god of Dilmun—and no other god. Furthermore, the word *Agarum* possibly refers to a very ancient Arab tribe of northeastern Arabia, a tribe whose name may have survived in *Hagar* (or *Hadjar*)—a term frequently employed in mediaeval times for El-Ḥasa and even perhaps for Baḥrein Island—and in the name of the modern Banū Hagar, a large tribe still dwelling on the Arabian mainland opposite Baḥrein (Cornwall 1944:74f.). At the side of the inscription, which is cut on a black basalt stone, there is a well-executed palm branch. The Dilmun dates were famous and are mentioned in numerous cuneiform texts.[8]

(5) That part of the East Arabian littoral was included in the kingdom of Dilmun is shown by inscriptions of Sargon of Assyria, wherein it is said that he brought under his sovereignty "Bît-Iakin on the shore of the Bitter Sea as far as the border of Dilmun." And that Bît-Iakin extented into Arabia—perhaps including much of what is now Kuweit—is the prevailing opinion of scholars.[9]

This historical evidence for Dilmunite control of the Arabian littoral is most important, especially in view of the next point to be considered.

(6) In 1940-41 I spent six months excavating and exploring on Baḥrein and in El-Ḥasa. I found that on the Arabian mainland opposite Baḥrein there exist thousands of tumuli of the same types as on the island itself. This fact, previously unknown, may be taken to indicate a close cultural—and probably a political—tie between the mainland and the island during the Bronze Age, just as in mediaeval times.

(7) In addition, cuneiform records give us considerable information on the goods imported from, or through, Dilmun. The contents of the tumuli excavated to date are quite in accord with this evidence (Cornwall, 1944:135ff. 94, note 7). It is also very likely, judging from mediaeval and present conditions in the Persian Gulf, that the famous Dilmun dates were in large part derived from the great mainland oases now called Qaṭîf and Hofûf. These places have long been renowned throughout the Arab world for the excellence of their dates.

(8) Finally, even Sumerian literary sources provide material in agreement with the identification. In particular, we seem to find in them references to the mysterious and unusual springs of Baḥrein (Haupt 1881:127, 11.35f).[10]

To sum up, the basic identification of Dilmun with Baḥrein Island depends not upon a single line, but upon several lines of evidence. As Albright once put it: "All evidence, literary, archaeological and onomastic, converges irresistibly towards the identification" (Albright 1925:237).

Dr. Kramer's argument for locating Dilmun in southwestern Iran is based on two principal points. He observes, first, that in the Sumerian "deluge" myth Ziusudra is caused by the gods to dwell in "Dilmun, the place where the sun rises" (Poebel 1914:IV, 12). He therefore considers that Dilmun must be sought east of Sumer, adding "It is hardly likely that the island of Baḥrein, hugging the Arabian coast, would be conceived by the Sumerians as lying east of their own land." At first sight this point seems reasonable enough, but it loses force when one recalls that a familiar Assyrian name for the Persian Gulf was "the sea of the rising sun" (Luckenbill 1926: Vol. 2, sect. 41, 70, 92 and 185; Cornwall 1944: Vol. 2, sect. 81 and 99).

Kramer's second point is that possibly we might so translate, or so interpret, certain passages in Sumerian literary compositions as to label Dilmun a "land of cedars"—a term admittedly inapplicable to Baḥrein.[11] But a close examination of the material which he advances in support of this claim will show how uncertain is the inference.[12] It may, indeed be questioned whether the passages drawn on—so rich in metaphor and imagery—are not too indeterminate in meaning to provide Dr. Kramer with a principal reason for preferring southwestern Iran as the location of Dilmun.

Moreover, we possess a list of articles sent by the wife of Lugalanda (*ensi* of Lagash c. 2500 B.C.) to Dilmun: wheat, cedar wood,[13] cheese, and shelled barley.[14] The need for the food items is understandable, for Baḥrein has little cultivable land and must import much of its food. But what of the cedar wood? I am confident that Dr. Kramer will not maintain that

Dilmun was a land of cedars and suggest that what we have here is the earliest example of "carrying coals to Newcastle."

Of the Baḥrein tumuli—which number in all perhaps fifty thousand[15]—I excavated thirty. On the mainland I opened only two small mounds, for political factors prevented any large-scale work. But since road-building operations carried out in Arabia by oil-men had cut into other mounds, I was able to ascertain that on the mainland as on the island the tumuli are of two main types.

First, there are small and compact rock tumuli. They average about 8 feet in diameter and 4 feet in height, although a few are considerably larger. Some conceal a crude cist; in others the rocks forming the core appear simply to have been piled haphazard. From these rock tumuli I obtained only fragments of human bones, red potsherds, and one bone point. Nearby were found on the surface chert spearheads of chalcolithic type.

Secondly, there are the larger and far more numerous mounds that are gravel-covered and contain at least one well-made tomb chamber. Occasionally these tumuli are two or three times the size of the rock mounds, and near the village of Ali, on Baḥrein, are a few immense "pyramid" mounds, as much as 82 feet in height and 100 feet in diameter at the base.

In the chambered mounds I discovered broken and unbroken pottery, mostly wheel-made and round-bottomed; pieces of ostrich shell and fragments of ivory boxes; a bronze spearhead of unusual shape; a bronze spatula (?); and three bronze points, each found in a different tomb. The Dilmunites presumably inserted these bronze points into hair that was worn in a bun, Sumerian style. I also obtained a series of ten nearly complete skeletons; they have not yet been examined by specialists, but it can be said that the mound-builders were dolichocephalic and rather tall. In most cases the body had been laid on its right side, with legs flexed and hands before the face. In one large tomb were the remains of two charpoys, or beds. In some chambers I found the bones of a ram or sheep.

The rock tumuli, which often resemble pill-boxes, usually are situated on higher ground—and lie further inland—than mounds of the other category. I came upon one isolated group on a hill, Er-Rudaif, 95 miles northwest of Qaṭîf. Chambered mounds, on the contrary, are generally clustered thickly near level ground and give the impression of having served as the cemetery of a settled and not unprosperous community.

I believe that the rock mounds are the forerunners of the chambered type, and that there is some connection between them and the similar mounds known to exist in central and southwestern Arabia (Philby 1939:371 and illustrations facing pp. 376, 378 and 382). It may well be that the mound-building Dilmunites were Semites who had migrated from the interior of Arabia to the Persian Gulf, where they came into contact with earlier settlers—Dilmunites who, if not actually of Sumerian descent, certainly had been influenced by Sumerian culture.

The date of the tumuli is still uncertain, but the bulk of them probably belong to the middle centuries of the second millennium B.C. Undoubtedly they were erected over a long period of time; and as the skeletal evidence implies that, as a rule, the mounds were constructed for adult warriors of the tribe, the dead buried within the tumuli represent only a fraction of the ancient population.[16]

NOTES

[1]The entire matter is treated in detail in Cornwall (1944).

[2]In my thesis on Dilmun (Cornwall 1944) I have shown that if the Sumerians originally came to Mesopotamia by way of the Persian Gulf, Bahrein would have been a principal stopping place, for it provides the only sheltered harbor and good fresh water on that route. Such an early connection between Sumerians and the little island would go far toward explaining the sacred character which Dilmun assumed in Sumerian tradition, and would make understandable not a few other matters which have long been obscure

[3]"Sea of the rising sun" was one Assyrian name for the Persian Gulf. Others, it will be recalled, were "Lower Sea" and "Bitter Sea."

[4]*Anabasis of Alexander*, Book VII, 20:6.

[5]See Meissner 1917:201-3; Albright 1925:238; Rawlinson, *Journal of the Royal Asiatic Society*, New Series Vol 12, 1880, pp. 215f.; Burrows, *Orientalia*, Vol. 30, 1928, p 3; and Deimel, *Orientalia*, Vol. 30, 1928, p. 30.

[6]It is given in *Journal of the Royal Asiatic Society*, New Series, Vol. 12, 1880, pp 189ff.

[7]King (1909.35, obv 20). Moreover, in line 277 of the Sumerian myth which Dr. Kramer entitles "Enki and Ninḫursang" Enšag (i.e. Inzak) is caused—apparently by Enki—to be lord of Dilmun, cf. *Bulletin of the American Schools of Oriental Research*, Supplementary Studies, No. 1, p. 21 and note to line 268.

[8]See Ailotte de La Fuye (1943.383, 385, 393, 395, 396, 403, 405, 408); Nikolsky (1908: sect 46, 47); and Dougherty (1932:114, note 353).

[9]Dougherty's comments (1932:53, note 158, 66f) are still very pertinent.

[10]See also *Bulletin of the American Schools of Oriental Research*, Supplementary Studies, No 1, 11.45ff. For a local Arab legend bearing an interesting resemblance to the latter incident, see Bent (1898).

[11]Neither can I find any material indicating that Laristan (where Dr. Kramer localizes Dilmun) was ever a "land of cedars," like Lebanon.

[12]For example, Dr. Kramer gives a passage from a Ninisinna hymn copied by Edward Chiera in the Istanbul Museum of the Ancient Orient:

My house, before Dilmun existed, was fashioned from cedar.
Isin, before Dilmun existed was fashioned from cedar.

He then remarks: "Now, the sign which I read above as *erin* (!) 'cedar' is actually GIŠIMMAR 'palm tree' not *erin*, the two signs resemble each other not a little, and because of the context, I felt justified in assuming that GIŠIMMAR is a miscopy for *erin*. . . ." But for the present it is surely better to retain "palm tree," the more so since we *know* that Dilmun was a great producer of dates. Even should "cedar" and not "palm tree" prove correct, the passage quoted would still be susceptible of more than one meaning. In point of fact, however, I understand that a demonstration of the correctness of Chiera's reading of GIŠIMMAR is forthcoming.

[13]It may be well to point out that in cuneiform writing of the third millennium B.C. the sign for *erin* was hard to confuse with that for GIŠIMMAR.

[14]See Thureau-Dangin (1903: sect. 26) and Fortsch (1916. 193f.). The sending of cedar wood to Dilmun is also mentioned in unpublished VAT no. 4804 (Staatliche Museen, Berlin).

[15]For the information of those who have not read the reports by Mackay or myself, I would mention that near the northern shore of Bahrein Island there are several score mounds different in appearance from the others. Excavating several of these, I found that each contained a number of burials dating from the early centuries A.D. The skeletons, which lay at full length in narrow plaster-and-rock cists, had with them jewelry, household implements, alabaster jars, etc. On the Arabian mainland I saw no mound of this sort; but it is possible that a few such mounds may exist somewhere in the Qatif area

[16]For details of archaeological work done on the Bahrein mounds during the present century see the following publications: Prideaux (1910), Mackay *et al* (1929), and Cornwall (1943, 1944, 1945, 1946).

Editor's note: In this paper Dr. Cornwall mentions that his dissertation (Cornwall 1944) was to be published in 1947. To my knowledge this never came to fruition It should be added that since 1946 there has been a great deal of additional archaeological work on Bahrein Island. See especially Glob (1958a and 1958b); Glob and Bibby (1960); Bibby (1958a, 1958b, 1961, 1962, 1969, and 1970). The Danish archaeological journal *Kumul* has also carried English summaries of much of this importnat work.

The Indus Civilization and Dilmun: The Sumerian Paradise Land

SAMUEL N. KRAMER

I

One of the most significant and impressive archaeological achievements of the twentieth century centers around the discovery of the ancient Indus civilization which probably flourished from about 2500 to 1500 B.C., and extended over a vast territory from the present Pakistan-Iran border to the foot of the Himalayas and to the Gulf of Cambay. Much of the material culture of this civilization is now known: the well-planned streets and structures of its cities and towns; its tools and implements of stone and metal; its pottery and vases; its beads and bangles; its stone sculptures and terracotta figurines. On the other hand, but little is known of the social, political, religious, and intellectual life of the ancient Indus people, and that little is based on conjecture and surmise. And practically nothing at all is known of its ethnic and linguistic affiliations; indeed the very name of this ancient Indus land is still a mystery.

To be sure, the Indus people did have a well-developed system of writing consisting of some four hundred pictographic signs with conventionalized syllabic values. Moreover, since the reading and writing of this script had to be studied and learned by budding scribes, there is every reason to assume that there were schools scattered throughout the land with a formal system of education. But the inscriptions recovered to date consist of very brief notations on steatite seals, clay sealings, pottery stamps, and small thin copper plates; usually they contain no more than half a dozen signs, and the longest has less than twenty. There is some likelihood that these inscriptions record the names of individuals and places, and if so, they could be most revealing for the ethnic origin of the Indus people. But as of today not a single sign of the Indus script has been deciphered and read satisfactorily, and until some bilinguals are discovered, the available Indus inscriptions will probably remain a closed "book."

There is, however, one possible source of significant information about the Indus civilization which is still untapped: the inscriptions of Sumer, approximately six hundred miles to the west of the mouth of the Indus and separated from the Indus land by the Arabian Sea and the Persian Gulf. That there was considerable commercial trade between the two countries is proved beyond reasonable doubt by some thirty Indus seals which have actually been excavated in Sumer—and no doubt hundreds more are still lying buried in the Sumerian ruins—and which must have been brought there in one way or another from their land of origin. There is, therefore, good reason to conclude that the Sumerians had known the name of the Indus land as well as some of its more important features and characteristics, and that some of the innumerable Sumerian texts might turn out to be highly informative in this respect.

With this in mind, I searched the Sumerian literary works for possible clues and came up with the tentative hypothesis that Dilmun, a land mentioned frequently in the Sumerian texts and glorified in Sumerian myth, may turn out to be the Indus land or at least some part of it. According to a long-known Sumerian "Flood"-story, Dilmun, the land to which Ziusudra, the Sumerian Noah, was transported to live as an immortal among the gods, is "the place where the sun rises," and was therefore located somewhere to the east of Sumer. In another Sumerian text, Dilmun is described as a blessed, prosperous land dotted with "great

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dwellings," to which the countries of the entire civilized world known to the Sumerians, brought their goods and wares. A number of cuneiform economic documents excavated by the late Leonard Woolley at Ur—Biblical Ur of the Chaldees—one of the most important cities of Sumer, speak of ivory, and objects made of ivory, as being imported from Dilmun to Ur. The only rich, important land east of Sumer which could be the source of ivory, was that of the ancient Indus civilization, hence it seems not unreasonable to infer that the latter must be identical with Dilmun.[1]

But promising and intriguing as it was, the Dilmun-Indus land hypothesis was the product of "arm-chair" scholarship, which needed corroboration from the "field," that is, from the extant archaeological remains of the Indus civilization. I therefore journeyed to Pakistan and India, with the help of a grant-in-aid from the American Council of Learned Societies, and in the course of a seven weeks stay there, travelled more than four thousand miles by plane, train, bus, automobile, and a horse-drawn vehicle known as the tonga, in order to visit the excavated Indus cities: Harappa, Mohenjo-daro, Kot Diji, Amri, Rupar, and Lothal. I studied the Indus artifacts located at the site museums, as well as the rich collections in the museums of Karachi, Lahore, and New Delhi. I met many of the archaeologists of Pakistan and India, and discussed with them the various aspects of the Indus civilization. As a result of these investigations and discussions, it became apparent that there are two facets of the Indus civilization which are especially significant for its identification with Dilmun: the cult of a water deity and sea-plowing ships.

One of the most striking and impressive features of the Indus cities and towns is the important role which water and cleanliness seem to have played in the life of the people, as is evident from the extraordinary number of wells and baths in both public and private buildings, as well as the carefully planned networks of covered drains built of kiln-baked bricks. It is not unreasonable to assume therefore—as indeed has been assumed by a number of scholars—that the Indus people had developed a water cult of deep religious import centering about a water god and featured by sundry rites concerned with lustration and purification. All of which seems to fit in rather surprisingly well with the Dilmun-Indus land equation. For the god most intimately related to Dilmun is Enki, the Sumerian Poseidon, the great Sumerian water god in charge of seas and rivers. Thus we find a Sumerian Dilmun-myth which tells the following story: Dilmun, a land described as "pure," "clean," and "bright," a land which knows neither sickness nor death, had been lacking originally in fresh, life-giving water. The tutelary goddess of Dilmun, Ninsikilla by name, therefore pleaded with Enki, who is both her husband and father, and the latter orders the sun-god Utu to fill Dilmun with sweet water brought up from the earth's water-sources; Dilmun is thus turned into a divine garden green with grain-yielding fields and acres. In this paradise of the gods eight plants are made to sprout by Ninhursag, the great mother goddess of the Sumerians, perhaps more originally Mother Earth. She succeeds in bringing these plants into being by the intricate process involving three generations of goddesses all begotten by Enki and born without pain or travail. But because Enki wanted to taste them, his messenger, the two-faced god Isimud, plucks these plants one by one and gives them to his master who proceeds to eat them each in turn. Whereupon the angered Ninhursag pronounces the curse of death against Enki and vanishes from among the gods. Enki's health at once begins to fail and eight of his organs become sick. As Enki sinks fast, the great gods sit in the dust, seemingly unable to cope with the situation. Whereupon the fox comes to the rescue and after being promised a reward, he succeeds by some ruse in having the mother goddess return to the gods and heal the dying water god. She seats him by her vulva and after inquiring which eight organs of his body ache, she brings into existence eight corresponding deities—one of these is Enshag, the Lord of Dilmun—and Enki is brought back to life and health.

Now the first part of this Dilmun myth reads as follows:

The holy cities—present them to him (Enki?),
The land Dilmun is holy,
Holy Sumer—present it to him,
The land Dilmun is holy.
The land Dilmun is holy, the land Dilmun is pure,
The land Dilmun is clean, the land Dilmun is holy;
Who had lain by himself in Dilmun—
The place, after Enki had lain with his wife,
That place is clean, that place is most bright;
Who had lain by himself in Dilmun—
That place, after Enki had lain with Ninsikilla,
That place is clean, that place is most bright.
In Dilmun the raven utters no cry,
The wild hen utters not the cry of the wild hen,

The lion kills not,
The wolf snatches not the lamb,
Unknown is the kid-devouring wild dog,
Unknown is the grain-devouring boar,
The malt which the widow spreads on the roof—
The birds of heaven do not eat up that malt,
The dove droops not the head,
The sick-eyed says not "I am sick-eyed,"
The "sick-headed" says not "I am sick-headed,"
Its old woman says not "I am an old woman,"
Its old man says not "I am an old man,"
Unwashed is the maid, no water is poured in the city,
Who crosses the river (of the Nether World) utters no groan (?),
The wailing priest walks not round about him,
The singer utters no wail,
By the side of the city he utters no lament.

The passage which follows is fragmentary; to judge from the preserved lines, it contained the goddess Ninsikilla's prayer to Enki to supply Dilmun with water. The poem then continues thus:

Father Enki answers Ninsikilla his daughter:
"Let Utu stationed in heaven,
Bring you sweet water from the earth, from the water-sources of the earth,
Let him bring up the water into your large reservoirs (?),
Let him make your city drink from them the water of abundance,
Let him make Dilmun drink from them the water of abundance,
Let your wells of bitter water become wells of sweet water,
Let your furrowed fields and acres yield you their grain,
Let your city become the 'dock-yard'-house of the (inhabited) land."

And just as Enki had spoken "so it came to be"; or in the words of the poet:

Utu stationed in heaven,
Brought her (Ninsikilla) sweet water from the earth, from the water sources of the earth,
Brought up the water into her large reservoirs (?),
Made her city drink from them the water of abundance,
Made Dilmun drink from them the water of abundance,
Her wells of bitter water became wells of sweet water,
Her furrowed fields and acres yielded her grain,
Her city became the "dock-yard"-house of the (inhabited) land,
Dilmun became the "dock-yard"-house of the (inhabited) land.

While not everything in this passage is clear, one fact stands out; Dilmun, not unlike the Indus land, was particularly noted for cleanliness and purity, and it was a water god who played a leading role in the religion of the two lands.

That Dilmun was a land characterized by purity and cleanliness is indicated by a passage in another Enki myth recently pieced together and translated, which may be entitled "Enki and the World Order." Part of this myth is devoted to Enki's "decreeing the fates" of the lands constituting the world known to the Sumerians. The passage involving Dilmun consists of six lines but only two are fully preserved and these read interestingly enough:

He (the god Enki) cleaned and purified the land Dilmun,
Placed the goddess Ninsikilla in charge of it.

In fact the very name of the goddess whom Enki placed in charge of Dilmun is a Sumerian compound word whose literal meaning is "the pure queen," another indication of the value put on cleanliness in Dilmun.

During the past few years, there have been uncovered in Pakistan several sites of ancient Indus towns which were originally located on the coast of the Arabian Sea, although as a result of coastal uplift, these are now some distance away from the edge of the sea. The existence of these settlements, taken in conjunction with the numerous long-known sites strung all along the Indus River, indicates clearly that the Indus civilization depended largely on water-borne trade, coastal and riverine. This is now corroborated by the excavations conducted over the past five years in Lothal, a site in India not far from the Gulf of Cambay, where what seems to be a well-planned rectangular dockyard built of baked bricks has been uncovered, complete with spillways, water-locks, and loading platforms—when I visited the site in January 1961, workmen were still trying to reach the bottom of its

solid embankments. Now this type of maritime civilization must have been characteristic of Dilmun, to judge from the Sumerian inscriptions in which "ships of Dilmun" are mentioned repeatedly. Thus, one of the Sumerian rulers by the name of Ur-Nanshe, who lived as early as about 2400 B.C., speaks of timber-carrying Dilmun boats arriving at his city, Lagash. Sargon the Great who ruled about a century and a half after Ur-Nanshe, boasts that the boats of Dilmun lay anchored at the docks of his capital city Agade. In the myth "Enki and the World Order" mentioned earlier, Enki boasts of the moored Dilmun boats. Ivory-bearing boats from Dilmun to Ur have already been mentioned; according to the texts these also carried timber, gold, copper, and lapis lazuli. No wonder that in the "Paradise" myth cited above, Dilmun is described as "dockyard-house of the (inhabited) land."

The Dilmun-Indus equation, if correct, will help to clarify the baffling problem of the origin and rise of the Indus civilization, especially in regard to the ethnic and linguistic affiliation of the Indus people. There is some reason to surmise that the rise of the Indus cities was in the nature of a cultural "explosion" or "revolution" due to the arrival in India of a new ethnic group which had already attained a high degree of civilization. For there is not too much in the remains of the pre-Indus settlements excavated at Harappa, Kot Diji, or Amri, which could be regarded as the forerunner of the Indus cities and towns with their carefully planned buildings and streets, their water cult and purification rites, their well-developed pictographic script, and their bustling water-borne trade. As for the time when this highly civilized people came to India to which it transplanted some of the skills and ideas developed in its original habitat, the likelihood is that it took place early in the third millennium, some time about 2800 B.C., since it must have taken several centuries for the Indus civilization to grow to the size it had become about 2500 B.C.

Now it is hardly likely that this people came to India from anywhere but Mesopotamia. For it is in Mesopotamia that we first find a fully developed urban civilization with monumental architecture, a pictographic script utilized for administrative purposes, and flourishing trade relations with neighboring countries by land and sea. It is in Mesopotamia, too, that we find the worship of a water god from earliest days; his main cult was in the city of Eridu where his first shrine dating from the middle of the fourth millennium B.C., or even earlier, was excavated more than twenty years ago.

But if so—if it was a Mesopotamian people who loaded their boats with their families and possessions, and abandoned their native homes to start life afresh in distant India—who was this people? Hardly the Sumerians, as has been suggested more than once. The Sumerian pictographic script of the early third millennium B.C. is now well known from excavations at Warka and Jemdet Nasr, and it bears little resemblance to that of the Indus seals. Moreover, why should the Sumerians who had themselves probably arrived in Mesopotamia only a few centuries earlier and made themselves lords and masters of the land later known as "Sumer," leave their homes where they lived as conquerors and rulers in search for a new habitat? On the face of it, it is much more likely that it was not the Sumerians, but one or another of the Mesopotamian peoples subjugated by the Sumerians, who, seeing their language, faith, and way of life threatened and perhaps even suppressed, decided that home was no longer home for them and went forth to establish themselves in a new land where they were free to live their lives in accordance with their religious convictions. The Mesopotamian people which settled in India and sparked the Indus civilization were therefore not the Sumerians but—most probably—the original settlers of "Sumer," the Ubaidians, as they have come to be known from the name of the Mesopotamian site where their archaeological remains were first identified.

If this should turn out to be correct—if it was the Ubaidians who created the Indus civilization—we now have some linguistic data which might prove of no little value for the Indus language and script. For while we still know practically nothing about the grammar and structure of the Ubaidian language, we do know a number of Ubaidian words denoting place names and occupations. The names of the two great Mesopotamian rivers, the Tigris and Euphrates, or *idiglat* and *buranun* as they read in the cuneiform texts, are Ubaidian—not Sumerian—words. So, too, are the names of the most important urban centers of "Sumer": Eridu, Ur, Larsa, Isin, Adab, Kullab, Lagash, Nippur, and Kish. In fact the word Dilmun itself may, like the word *buranun* for the Euphrates, be Ubaidian. More important still, such culturally significant words as *engar* (farmer), *udul* (herdsman), *shupeshdak* (fisherman), *apin* (plow), *apsin* (furrow), *nimbar* (palm), *sulumb* (date), *tibira* (metal worker), *simug* (smith), *nangar* (carpenter), *addub* (basket maker), *ishbar* (weaver), *ashgab* (leather worker), *pahar*

(potter), *shidim* (mason), and perhaps even *damgar* (merchant), are probably all Ubaidian rather than Sumerian, as has been usually assumed. And should the inscriptions on the Indus seals contain not only the name of the consignor or consignee of the goods to which their clay impressions were attached, but also his occupation. it is not impossible that one or another of the above listed words will be found among them.

Another crucial word which may turn out to be Ubaidian, is Ea, one of the two names by which the Mesopotamian water god is known in the cuneiform texts, the other being Enki, the name used throughout this study. For while the latter is a typical Sumerian compound with the meaning "Lord of the Earth," Ea is a word whose linguistic affiliations are still uncertain; it might well be his original Ubaidian name which the Sumerians changed to Enki when they incorporated him into their pantheon. This is corroborated to some extent by the fact that, to judge from the hymns and myths, the Sumerian theologians found it necessary to stress and explain repeatedly the source of Enki's authority and power; in fact Enki often talks and acts as if he had an inferiority complex. If it is the Ubaidians who brought the water cult to India, Ea would be the name of the god about whom it centered, and it would not be too surprising to find the name in one or another of the Indus seals.

For well-nigh a thousand years following the collapse of the Indus civilization, the history of India is practically a blank, archaeologically speaking. If however, the Dilmun-Indus equation should prove to be correct the cuneiform documents from Mesopotamia would give us at least a glimpse into this Indian "dark age." For throughout the latter half of the second millennium and the first half of the first millennium B.C., we find Dilmun mentioned in the cuneiform documents. The Assyrian king Tukulti-Ninurta uses in his titles the expression "king of Dilmun and Meluhha" which is reminiscent to some extent of the Biblical "from India to Ethiopia" of King Ahasuerus in the Book of Esther. There was a king of Dilmun by the name of Uperi who paid tribute to Sargon II of Assyria. There is another king by the name of Hundaru in whose days booty taken from Dilmun consisted of objects made of copper and bronze, sticks of precious wood, and large quantities of kohl, used as an eye-paint. A crew of soldiers is sent from Dilmun to Babylon to help King Sennacherib raze that city to the ground, and they bring with them bronze spades and spikes which are described as characteristic products of Dilmun.

But Dilmun or not, it is clear from the preceding pages that what is urgently needed is further intensive excavation of the Indus sites, especially the larger ones which may be expected to yield the inscriptional material essential for the decipherment of the Indus script—not only the Indus seals, but also the larger and longer documents which must certainly have existed, and perhaps even a bilingual written partly in cuneiform. Most of this promising work, naturally enough, will fall to the happy lot of the archaeologists of Pakistan and India. But Indus archaeology offers a rare and rich opportunity for American institutions of learning to help unravel the history of the Orient in the third millennium B.C.

II

The following is a translation from the Sumerian "Flood" tablet.

Life like a god they (the gods An and Enlil)
gave him,
Breath eternal like a god they brought down
to him.
Then, Ziusudra, the king,
The preserver of the "name" of vegetation
and the seed of mankind,
In the land of crossing, the land Dilmun, the
place where the sun rises, they caused to dwell.

In the next passage, from the myth "Enki and the World Order," the god Enki boasts of the moored Dilmun boats.

The lands of Magan and Dilmun
Looked up at me, Enki,
Moored(?) the Dilmun-boat to the ground(?),
Loaded the Magan-boat sky high.

The following are the initial lines of the Dilmun myth.

The holy cities—give(?) them to(?) him
(Enki?) as his share,
The land Dilmun is holy,
Holy Sumer—give(?) it to(?) him as a share,
The land Dilmun is holy.
The land Dilmun is holy, the land Dilmun
is pure,

The land Dilmun is clean, the land Dilmun
is holy.

Timber carrying Dilmun boats are noted in the following inscription of Ur-Nanshe.

The ships of Dilmun,
from the foreign lands,
brought him (Ur-Nanshe)
wood as a tribute(?).

Sargon the Great boasting that boats of Dilmun, Magan and Meluḫḫa lay anchored at the docks of Agade, his capital.

The ships from Meluḫḫa,
the ships from Magan,
the ships from Dilmun,
he made tie up alongside
the quay of Agade.

NOTE

[1]To be sure Dilmun has been identified by most scholars with the island of Bahrein in the Persian Gulf, and a large and highly competent expedition has been excavating there for close to a decade because of its faith in the identification (Glob and Bibby: 1960) But the discoveries at Bahrein to date have hardly justified this faith, and the description of Dilmun as "the place where the sun rises" in the Sumerian "Flood-story" speaks definitely against the Bahrein-Dilmun identification.

Contacts between Lothal and Susa

S.R. RAO

During the years 1954-60 the writer undertook a systematic survey of the coastline extending from the north-west corner of Kutch to the southern borders of Gujarat in western India as a result of which several Harappan settlements were located at the mouths of the rivers. The most important among them is Lothal. Situated as it is between the rivers Bhogawa and Sabarmati at the head of the Gulf of Cambay it provided a sheltered harbour to the seafaring Harappans engaged in trade with peninsular India on the one hand and the Indus Valley and Sumer on the other in the third and second millennia B.C. So extensive was the foreign trade of the Lothal folk that they had to build an artificial brick structure measuring 219 × 37 m. to receive ships and handle cargo in their busy port. (Rao 1963a:178-81. See Fig. E.)

The constructional and other details of the dock at Lothal which happens to be the earliest in the world have been mentioned elsewhere (Rao 1962). The main purpose of this article is to focus the attention of the scholars on the less-known aspect of the trade and cultural contacts between Susa on the one hand and the Indus sites, especially Lothal, on the other. Hence the writer craves the indulgence of the scholars for omitting references to other West Asian sites, except by way of a casual mention here and there.

Two cultural periods namely, A and B, with four structural phases in the former and one in the latter, have been distinguished at Lothal. Period A dated to 2450-1900 B.C. represents the mature phase of Harappa culture and Period B dated to 1900-1600 B.C. represents the degenerate or Late Harappa culture. So far as Susa is concerned McCown's sequence is followed (1942). Hence, contacts between Susa D (which used to be called Susa II) and Lothal A are discussed here. The early levels of Susa D are contemporary with Early Dynastic Sumer while the late levels extend into the Akkadian period. There are definite evidences in the form of a Persian Gulf Seal (Rao 1963b), the Reserved Slip Ware and other knick-knacks to prove that Lothal had a flourishing sea-borne trade with the west in the Sargonid period, if not earlier.

In the course of a detailed study of the excavated finds from Susa in the reserve collections of the Louvre Museum in Paris it was observed that some of them, notably the ingots and amulets of copper, the terracotta, gaming pieces, the cubical stone weights and painted pottery, bore close resemblance to those from Lothal. A brief mention of the analogous objects is made below.

Copper and bronze objects. Lothal, Mohenjo-daro and Harappa have all yielded bun-shaped ingots of copper varying in content from 99.87 per cent purity in the case of Lothal, to 97.07 per cent in the case of Mohenjo-daro (Mackay 1938b:458). Scores of similar ingots occur at Susa also. The Lothal specimen is almost plano-convex in section with a slightly concave undersufrace and has a puckered upper surface. Its diameter is 11 cms. and the maximum thickness 4.57 cms. Some of the ingots from Susa are identical in shape and size with those from Mohenjo-daro and Lothal. Between Elam on the one hand and the Indus Valley and Lothal on the other bun-shaped ingots have been found at Ras-al-Qala and Kuwait.[1] The Persian highlands and the Oman were important sources of copper in the third and second millennia B.C. As there is no evidence of working the copper mines in Rajasthan in the protohistoric period it is most likely that the Indus people imported copper from overseas

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sources. The identity in shape and size of the ingots found at Lothal, Mohenjo-daro, Bahrain islands and Susa may suggest a common source but the impurities present in them vary; there appears to have been a common channel of trade. Oppenheim quotes the clay tablets referring to the boats carrying copper ingots called *guburam* in Sumerian from Dilmum, etc. That the Sumerian merchants insisted on good quality and full weight of ingots is evident from a dispute between a Sumerian merchant and a supplier from Dilmum mentioned in the clay tablets from Ur (Oppenheim 1954). The extensive trade in copper in the second millennium B.C. is attested by the Bronze Age ship wreck recovered recently near Cape Galedonia off the Turkish coast. The Persian Gulf islands, Elam, Sumer and the Indus Valley must have been included in the orbit of the international trade in copper. Lothal and Susa too were active participants in this trade so much so that Lothal had to pay in shell, ivory, gemstones etc., for copper. Other trinkets were also exchanged. For example, a copper figurine of a dog and an amulet with the figure of a crouchant bull found at Lothal bear close resemblance to those from Susa. The amulet in particular seems to be an import from Susa. Among other finds mention may be made of barbed arrow-heads made of thin sheet, fish-hooks, hollow-drills and nails common to Lothal and Susa. The occurrence of a cubical chert weight and an Indus seal at Susa is highly significant. The Elamites and Sumerians used barrel or duck-shaped weights whereas cubical ones were exclusively used by the Indus people. Obviously the Indus weight and seal must have travelled from Lothal or the Indus Valley to Susa in the course of trade. Another important evidence of exchange of goods between Susa and Lothal is the occurrence of a terracotta sealing with compartmented squares resembling the design on the seals from Susa D and Giyan IV. Mention should also be made of the etched carnelian beads, disc-type wafer beads of steatite and inlays of shell and ivory of Indian origin found in Susa. On the other hand, the terracotta gamesmen with animal heads and small pellets from Lothal seem to be a copy of similar ones in stone and terracotta from Susa. One of the game boards from Susa is comparable with a fragmentary board found at Lothal. The similarity of games played in both the regions may also suggest exchange of ideas. Apart from the exchange of goods Lothal and Susa had cultural contacts too. Elsewhere it is mentioned that the Lothal potters developed a provincial style of painting as distinct from the characteristic Indus style which was also in vogue in Kathiawar (Rao 1962). Both in composition and motifs the provincial style appears to have been influenced by the Susan style of painting birds and plant motifs. Overcrowding and combination of geometric and natural motifs so frequently noticed in the Indus style are conspicuous by their absence in the provincial style of Lothal. On the other hand, one's attention is focussed on the individual plant motifs or the row of birds on Lothal pottery especially the Micaceous Red Ware which itself resembles to some extent the Red Ware from Susa in treatment and forms. The introduction of animals in their natural setting in painting and earthenwares at Lothal is more due to its contact with Elam and the Diyala region. Another point that may be noted here is the snake motif painted on the Lothal pottery. On one of the sherds a snake is shown entering an ant-hill while the other is coming out of it. In another case, two snakes are standing almost erect below a tree. Similarly two snakes are represented on a stele from Susa. It is therefore quite likely that snake-cult was common to Lothal and Susa. Snake sacrifice is reported from a Bronze Age site in Bahrain also.

From the above cited instances of exchange of goods and ideas between Lothal and Susa it can be inferred that they had close contacts in the Sargonid and perhaps pre-Sargonid periods too.

NOTE

[1]Information kindly given by Dr. G. Bibby.

Part VI The Major Settlements of the Indus Civilization

Editor's Introduction

A close reading of the original site reports is obviously the best exposure to the major settlements of the Indus Civilization. These reports, principally by Marshall (1932), Mackay (1938b and 1943), Vats (1940) and Khan (1964), are the primary sources for individual settlements. Since they are widely available it is not necessary for abstracts to be duplicated here. There is, however, another genre of literature which expands material in the original sources or reports major findings not yet finally published. It is contributions such as these that have been selected for this part.

The first three papers deal with Mohenjo daro and Harappa. The initial report by Sir John Marshall could have easily been placed with the more historically oriented papers in Part III. However, the historiographical issues notwithstanding, this report on the first seasons of excavation is important because it sets forth, in outline form, Marshall's approach, or research strategy, for the excavation of the two cities. The oft-noted failings of the final publications are not a reflection of an inadequate plan but rather to the mammoth size of the task Marshall had set for himself. Operating within sometimes severe budgetary and time limitations, he attempted to uncover as much of the city as was possible, to offer the first view of South Asia's earliest civilization.

One of the major criticisms which has been leveled against Marshall's work at Mohenjo daro is that he excavated the site in arbitrary levels. Time has proved this to be one of the most significant limitations in understanding both aspects of development within the city and in reconciling the stratigraphic relationships between the major excavation areas, DK, HR, VS and the Stupa Mound. Piggott (1948) attempted such a reconciliation which is critically assessed by H.T. Lambrick in his paper republished here. Lambrick's work is not only more detailed than Piggott's, in that he is able to distinguish more lucidly the features of the site, but it also goes well beyond the kind of stratigraphic presentation which is to be found in either the Marshall or Mackay site reports.

The remains of Mohenjo daro, as we know them today, were largely uncovered by Marshall and Mackay between 1922 and 1931. Since then there have been only two minor field seasons at the site: Sir Mortimer Wheeler in 1950, and George Dales in 1964. Neither of these short excavations has been fully published; however Dales' paper from *Archaeology* does outline his findings. Of greatest significance here is his uncovering of what seems to have been a massive wall on the southwest corner of the Lower Town adjacent to the HR excavation area. It is not known whether this wall was a part of an ancient fortification system, was meant to protect the city from floods or was constructed as a retaining wall necessary as the settlement grew above the surrounding plain. His wall is only a fragment at this point and much more of this feature must be exposed to answer the question. It is also interesting to note that Dales recovered unambiguous evidence for the extensive use of brick and timber construction in this part of the city.

Kalibangan is one of the most interesting and completely excavated Harappan settlements. B.K. Thapar, who helped direct the Kalibangan project, presents here a summary of the site as a metropolis beyond the Indus Valley. It can be added that the chronology of Kalibangan, and the relationship of the Pre- or Early Harappan levels there to other sites has yet to be fully understood. R.C. Gaur (1974) in the course of a discussion of the Ochre Colored Pottery from Northwestern India does, however, offer an

interesting and provocative model for these relationships which ought to be put to a test.

My paper on Lothal is included to expand an appreciation of the role this settlement played in the larger Harappan economic system. S.R. Rao's views on Lothal have been presented in earlier parts, as well as in his important book on the site (1973). Leshnik's response to Rao's hypothesis that Lothal was a sea port has been included here as well. But port or not, there are other issues of interest and importance which can be addressed with the Lothal data. One of these has to do with the way in which the Indus Civilization gained access to raw materials lying outside its presumed area of direct control. I have suggested that this was done through the establishment of an economic exchange with the hunters and gatherers who inhabited the frontiers of the civilization: a proposition which can stand or fall whether Lothal was, or was not, a sea port.

Other excavated settlements of the Indus Civilization—Chanhu daro (Mackay 1943), Amri (Casal 1964a), Kot Diji (Khan 1965), Saraikhola (Halim 1972a and 1972b), Jalilpur (Mughal 1974)—do not have a coherent body of secondary literature associated withthem. It will therefore be necessary for those interested in them to go directly to the reports on these sites for insights into their significance in our understanding of the Indus Civilization.

Harappa and Mohenjo Daro

SIR JOHN MARSHALL

In the field of exploration, it is natural this year to give the premier place to the remarkable discoveries made by the Department in Sind and the South West Punjab; for it is safe to say that no such epoch-making discoveries have ever fallen to the lot of an Archaeologist in this country. Hitherto India has almost universally been regarded as one of the younger countries of the world. Apart from palaeolithic and neolithic implements and such rude primitive remains as the Cyclopean walls of Rajagriha no monuments of note were known to exist of an earlier date than the 3rd century B.C., when Greece had already passed her zenith and when the mighty empires of Mesopotamia and Egypt had been all but forgotten. Now, at a single bound, we have taken back our knowledge of Indian civilization some 3000 years earlier and have established the fact that in the 3rd millennium before Christ and even before that the peoples of the Punjab and Sind were living in well-built cities and were in possession of a relatively mature culture with a high standard of art and craftsmanship and a developed system of pictographic writing.

The sites where these discoveries have been made are at Harappa in the Montgomery District of the Punjab and at Mohenjo-daro, more than 400 miles away, in the Larkana District of Sind. The former of these two sites has long been known to archaeologists as the find-place of a certain unique class of seals engraved for the most part with the effigy of a bull and bearing inscriptions in an unknown pictographic script. More than half a century ago some specimens of these seals were obtained by Sir Alexander Cunningham and published in his Report for 1875. Other specimens were subsequently acquired by the British Museum and published by Dr. J.F. Fleet in the *Journal of the Royal Asiatic Society* for 1912. But, though examples of these seals were thus well known to Orientalists, and must have been constantly seen by Mesopotamian experts in the British Museum, the secret of their age and character had baffled all investigators until a few months ago when the fresh materials available from Harappa and Mohenjo-daro enabled the riddle to be partially solved. At Harappa itself excavations had been started at my request by Rai Bahadur Daya Ram Sahni in 1920-21 and the results obtained by him were described in his Report for that year. For lack of funds, however, the operations could only be conducted on a very limited scale and the finds were correspondingly meagre. During the past year the excavations have again been resumed and with decidedly more fruitful results as will be apparent from the summary of them given below. The site of Harappa though manifestly that of a great city covering a vast area and containing many strata of successive buildings, will probably never prove so lucrative as that of Mohenjo-daro, for the reason that it was further removed from the main center of the Indus culture in Sind, and it cannot, therefore, be expected to have been so rich in articles of luxury. Like Harappa, Mohenjo-daro has also been known to the Archaeological Department for many years past. But until our recent excavations there was no suspicion that the remains which lay buried there dated back much earlier than the Kushan kings, to whose age belonged a large number of coins found on the surface of the site as well as the casing at any rate of the ruined monument which crowns its highest point. The excavation of this site was begun by Rakhal Das Banerji in 1921-22, and it is to him that we mainly owe

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the subsequent discoveries that have been made there. But owing to his continued illness no official account of his operations could be published in the Report for that year; and indeed, it was not until the summer of 1924 that an opportunity was given me of seeing the collection of antiquities recovered by him, which I then ordered to be brought to headquarters and compared with the finds from Harappa. That the finds from the two sites belonged to the same stage of culture and approximately to the same age, and that they were totally distinct from anything previously known to us in India was at once evident. So impressed indeed was I by their novel character that I lost no time in publishing an account of them in the *Illustrated London News* (1924), my hope being that through the medium of that widely read journal I might succeed in getting some light thrown on their age and character by archaeologists in other countries. This hope, I am glad to say, was at once fulfilled. In the following issue of the *Illustrated London News* appeared a letter from Professor Sayce (1924) pointing out the close resemblance between these objects from the Indus Valley and certain Sumerian antiquities from Southern Mesopotamia, and a week later there appeared in the same journal a longer article from the pens of Gadd and Sidney Smith (1924) giving a more detailed comparison of the pictographic scripts and other antiquities found in the two countries. Some of the analogies suggested by these two writers are fanciful, but most of them are undoubtedly correct and there can now no longer be any doubt that the Punjab and Sind antiquities are closely connected and roughly contemporary with the Sumerian antiquities of Mesopotamia dating from the 3rd or 4th millennium before Christ. Simultaneously also the same conclusion was reached by Dr. E. Mackay, Director of the American Expedition at Kish, who in an unpublished letter to me pointed out the similarity between the ceramic wares found at Mohenjo-daro and at Kish, and also brought to my notice that a seal identical with those found at Harappa and Mohenjo-daro had been discovered in the debris beneath a temple of Hammurabi's time.

Now that these important facts of their age and affinities have been established, much interest is naturally concentrated on the discoveries at Harappa and Mohenjo-daro, and there has been not a little wild writing in the Press on the subject. It is very desirable therefore that a full and authoritative account of the excavations up-to-date should be published as soon as possible. On the other hand, it is evident from my perusal of the reports written by the excavators that many pitfalls are likely to be encountered and that it will be safer not to issue any detailed Memoirs on the subject until the excavations have progressed further and we can feel our way with relative certainty in this new and unexplored field. For this reason I shall probably postpone the publication of the work hitherto accomplished until the close of the season 1925-26, by which time I anticipate that many of our present doubts and difficulties will have been solved. Meanwhile the brief summaries given below of the past year's operations will suffice to give an idea of how the work is progressing. One feature of these remains which seems to me to emerge clearly from the facts before us is that the civilization of which we have now obtained this first glimpse was developed in the Indus Valley itself and was probably as distinctive of that region, as the civilization of the Pharoahs was distinctive of the Nile. In the marvellous forward progress which mankind made during the neolithic, copper and bronze ages, the great river tracts of the then inhabited parts of the world played a most important part; for it was in these tracts that conditions were found most favorable for supporting a dense and settled population—namely: fertility of the soil, an unfailing water supply, and easy communications; and it was, of course, among such large and settled populations that civilization had the best chance of making progress. The debt which in the early stages of its development the human race owed to the Nile, to the Danube, to the Tigris and to the Euphrates is already well known. But how much it owed to the Indus and to the Ganges, has yet to be determined. But, though the civilization of the Indus will probably be found to have had its own distinctive characteristics, I surmise that it will also be found to have formed part and parcel of a much wider sphere of culture which embraced not only Southern Mesopotamia and India, but probably Persia, and a large part of Central Asia as well, and which may even have extended as far West as the Mediterranean—where the early Aegean civilization presents certain somewhat similar features.

What these discoveries in India may ultimately lead to, no one can at present foresee; but it is hardly possible to exaggerate their importance for the ancient history of this country. Even within the borders of Mesopotamia itself, the study of Sumerian culture is still at an early stage, and numerous problems await solution. Of the many known sites where Sumerian culture is represented very few have been adequately

explored, and although much knowledge has already been gleaned from them, it is clear that there is much more still awaiting the excavator. The majority of scholars hold that the Sumerians, who are on all hands admitted to be entirely distinct both linguistically and in other respects from all other races in that region, were an intrusive element in the population; and various attempts have been made in recent years to derive them from one region or another outside and to the east of Mesopotamia. The fact that at Harappa and at Mohenjo-daro, where the present materials were discovered, seals, etc., of Sumerian type are found to lie near the surface in city sites covering very large areas widely separated and remote from the sea, with clear evidences of multiple strata lower down proves that, whatever the history of the Sumerians in Mesopotamia may have been, a culture closely akin to theirs must have been widely disseminated in the Valley of the Indus, and have undergone a development reaching back incalculable centuries on Indian soil. If, therefore, those scholars are right who consider the Sumerians to have been an intrusive element in Mesopotamia, then the possibility is clearly suggested of India proving ultimately to be the cradle of their civilization, which in its turn lay at the root of Babylonian, Assyrian and Western Asiatic culture generally. It is obviously far too early, however, to regard this as more than a reasonable hypothesis, but the mere stating of the problem shows what fascinating vistas are now being opened up, and emphasises the need for pushing on with the exploration of the Indus sites on an extended scale and with as little delay as possible.

Among the many problems suggested by the new discoveries one of the most interesting will be that of the script in which the seal legends are written. It is manifest from the formation of the characters themselves that originally the writing was a pictographic one, one of the commonest characters for example on our Indian seals bearing still the obvious likeness of a fish. In the Sumerian usage of Mesopotamia, each one of these characters is said originally to have represented a single word, without phonetic element, but pronounced as a monosyllable, the characters being thus analogous to a numerous class of Chinese ideographs in use to-day in the Far East. At a later period the characters appear to have been used by the Semitic peoples in those regions not only in their pictographic but also in their phonetic value, thus giving us an interesting parallel to the condition of affairs still obtaining in Japan to-day, where the Chinese characters are sometimes given both the meaning and the (approximate) sound of the original Chinese, sometimes only the meaning of the original with the pronunciation of the corresponding Japanese word, while sometimes only the Chinese pronunciation of the character, divorced from its meaning, is used phonetically as a sort of syllabary wherewith to write the polysyllabic words of the local language. That this multifarious application of the characters composing a script may lead to complications rendering its decipherment extraordinarily puzzling at times, is obvious, and it will be no easy problem to determine which method of reading the characters is to be followed in the case of our Indian records from Harappa and Mohenjo-daro, as we have at present no means of determining whether the script was used in India in its pictographic or its phonetic value or in a combination of the two. In India the materials are at present too scanty to permit of even this initial problem being dealt with and it is essential that they should be augmented as extensively as possible, if we are to make progress in the direction of their interpretation.

Let it be added that the funds available this year have been so meagre that the excavators have been able to do little more than dig a few more trial trenches on the surface of the vast mounds at Harappa and Mohenjo-daro. What is now required at both these sites and at others of the same age in Sind and the Punjab and Baluchistan is a well organised and comprehensive campaign of excavation conducted on a scale comparable to that attained at Knossos. From a digger's point of view, Mohenjo-daro is an almost ideal site, and when adequate funds are forthcoming for its excavation there is no doubt that it will prove a rival to that or any other site of the prehistoric age.

At Mohenjo-daro work was confined this year to an altogether new mound, to the north of the modern cart track, which in contradistinction to the sites previously excavated was occupied almost entirely by secular buildings. After clearing the surface of brushwood and jungle digging operations were begun by sinking a 10′ trial trench in the centre of the mound running east and west. Before two feet of earth had been removed a number of brick walls were exposed with cross walls at intervals indicating the existence of a series of rooms. The excavation was then deepened till the floor level of the rooms was reached, roughly at a depth of 7 feet below the original ground level. Besides giving an idea of the nature of the buildings to be found here this first

trial trench also yielded a variety of interesting minor antiquities, including three square seals of steatite engraved with the device of a rhinoceros or unicorn and bearing a few letters in the Indo-Sumerian pictographic script, already familar from other finds from this site as well as from Harappa.

In a second trench sunk at right angles to the first one and running north and south a well with a system of connected masonry drains and water channels was exhumed. Excavation here was extended to a distance of 160 feet north of the first trench and brought to light a succession of well built domestic dwellings. The very limited funds available for excavation at Mohenjo-daro did not permit of an all round extension of the trial trenches so as to expose the complete structures to which the walls and rooms belonged. Digging operations were, however, extended along the south of the main trench, to a distance of 40 to 50 feet west from the spot where the well and bath drains had been discovered. An idea of the nature of the remains here uncovered can be obtained from a view taken from the west and another taken from the north-east. Portions of five buildings were brought to light in this area separated one from the other by narrow streets. That they served as residential houses is obvious, and one of their most striking features is the substantial character of their construction. The walls are generally built of solid brick masonry in mud mortar the size of bricks in common use being 11″ × $5\frac{1}{2}$″ × $2\frac{3}{4}$″ the ratio of the length, breadth and thickness thus being 4:2:1, which is admirably suited for the purpose of bonding. Two well baked bricks of larger dimensions measuring respectively 17″ × $8\frac{1}{2}$″× 3″ and 16″ × 8″× $2\frac{1}{4}$″ were met with in the course of digging, but they cannot be definitely assigned to any of the existing buildings. In two blocks a number of niches measuring from 2′ to 2′ 9″ in length and 9″ to 1′ in depth, have been left in the thickness of the walls. The floors of several rooms were paved with brick tiles, the pavement being often sub-divided by brick-on-edge partitions. Rectangular masonry piers measuring from 5′ to 7′ in length and 3′ to 4′ in breadth were discovered in four places and were probably the bases of pillars on which the roof was supported.

The site of Mohenjo-daro is proving very rich in finds and the present year's excavations have been particularly prolific in certain classes of minor antiquities. Especial interest attaches to the discovery of the square steatite seals of the Harappa type of which ten more specimens were secured this year as against three discovered at the same place during the excavations of 1922-23. They are of various sizes ranging from 9/10″ sq. to 2″ sq. and in most cases bear the effigy of a bull and a pictographic legend. Among other antiquities recovered at the site may be mentioned a large number of chert implements, a collection of shell and mother-of-pearl objects, terracotta toys, and painted pottery of different shapes and patterns, various kinds of domestic objects of stone and copper, and a number of polished black stone cubes probably used as weights.

The exploration of the ancient mounds at Harappa was resumed again in the season under review and good headway was made with it. It is evident, however, that a great deal of further excavation will be necessary before we can hope to gain anything like a clear understanding of the unfamiliar monuments with which we are dealing. The first task during the past season was the expansion of a part of the long trench drawn across site F in 1920-21, where certain well-preserved walls pointed to the possible survival of a large dwelling. On the resumption of work at this point one of the walls referred to was followed up for a length of about 80 feet. There were also two other walls in a fairly good condition running parallel to it on either side. These, however, turned out to be all that had survived of the buildings. That the structure was an important one is evidenced by several interesting objects which came to light in it. In 1920-21 it had yielded a well-preserved seal and a pair of faience bangles of excellent workmanship; and it has now given us three more seals and the painted neck of a large earthen jar. The decoration on the latter, which was executed in black before the vessel was burnt, consists of a broad band with three rows of interlaced circles and a scalloped border at either end. The patterns beneath this band represent reeds or ears of corn. Of the seals found in this area the best preserved is No. A336. It bears the figure of a unicorn standing to left and an inscription in the same "Indo-Sumerian" characters as are found on other seals from Harappa and Mohenjo-daro.

Four other trial trenches were dug in site F in places which appeared most favorable for examination. One of them to the west of the main trench disclosed at the depth of about 8 feet a well-preserved water reservoir lined with brick and provided with a narrow covered channel. By the side of the tank was standing a large earthen jar with its pointed bottom fixed in the ground. It was filled with earth in which were several smaller

jars and terracotta figurines. The exact purpose of the reservoir has not yet been ascertained. In one of the sites explored at Mohenjo-daro in Sind, Mr. Banerji brought to light a tank analogous to the one found at Harappa and is of opinion that it was used as *charanamritakunda*, i.e., a receptacle for the holy water used for the washing of the sacred image, but in face of our almost complete ignorance of the people and religion to which these remains belong this opinion cannot be regarded as more than a mere surmise. Another trial trench which had to be cut through a thick layer of earth and débris revealed a huge mass of broken earthen vessels of different shapes and designs varying in size from small earthen bowls and crucibles to large jars of the type referred to above. These vessels must undoubtedly have been used for domestic purposes. Other portable objects found here included three seals, two of which are composed of faience and contain only geometric patterns. The third seal is in ivory and contains a legend of one line without any animal device. What appears to be a faience chess-piece also deserves mention. The structural remains brought to light in this trench consisted of two thick walls running parallel to each other at a distance of about 2 feet. They are broken at both ends and contain no openings for doors. These walls came to light at the depth of 11 feet below the surface but above them and intersecting them at right angles were two others that must be assigned to a considerably later date. Two other trenches at this spot revealed a corner of a brick building in which two earthen ware vessels containing human bones lay buried. These are the only urns so far met with at Harappa.

The operations were then shifted to the mound marked A—B in General Cunningham's plan, on the east side of which some trial diggings had been carried out in the year 1920-21. The object of this year's work was to obtain a general idea of the stratification of this lofty mound before operations on a larger scale were embarked on. For this purpose a trench about 140 feet long and 25 feet wide was drawn in continuation of the previous excavations referred to above. Two other trenches were also sunk at right angles to it towards the west. Owing to the deep accumulation of earth and insufficiency of funds the virgin soil could not be reached anywhere. A portion of the main trench was, however, carried to a depth of some 20 feet above the surrounding fields or about 35 feet below the highest point of the mound. This resulted in the determination of as many as seven successive layers of buildings indicating a very prolonged occupation of the site with, no doubt, other strata still unexposed below them. In the topmost stratum, which occurred at the depth of 13 feet from the summit of the mound, was revealed a round brick granary standing about six feet high. By the side of the granary was lying an earthen vase, $5\frac{3}{4}''$ in height, and painted in black colour with a row of four winged antelopes with long antlers stretching back over their bodies. The next three strata were devoid of any structural remains though the lowest of them was packed with a large number of undulating stone rings of the same character as those described by General Cunningham. What purpose they served remains a mystery. In the fifth stratum from the top two brick structures were observed. One of these is a square room with thick walls and paved interior which was divided into two smaller chambers at a later date. A narrow covered drain in its south wall and two low masonry benches on the inside seem to point to its having been used as some sort of an image shrine though it is difficult as yet to say whether idol worship existed in those early times. In this connection it is interesting to observe that the operations of 1920-21 brought to light at this very spot the defaced terracotta base of a statuette which might have been an object of worship. The other structure found on this level is a confused mass of brick remains in which a thick wall, thirty feet in length, resting partly on a solidly built platform stands out prominently. A small square seal of white plaster in perfect preservation was sticking to this wall. The next lower stratum is represented by the somewhat better preserved structure which had been laid bare in this area in 1920-21 and a number of tunnels left by the brick diggers. The seventh stratum will probably be found to be occupied by a building of considerable magnitude though so far only a portion of its floor has been exposed. This floor consists of two courses of brick laid as stretchers and headers on a strong substratum of pounded brick. As it was considered inadvisable to dismantle this structure, the examination of the earlier strata was postponed to the next season when it is proposed to extend the operations to a wider area. Several small objects were found on the floor mentioned above, the most interesting of which is a seal in fine-grained yellow steatite with a deep cut inscription, but no device.

The sites F and A—B and some of the other mounds bid fair to yield many more valuable relics and the deep ravines cut by rain water afford convenient places for reaching the earlier strata with comparatively little

trouble. One of these ravines immediately to the south of mound A—B was partly explored during the recent operations and disclosed several interesting relics. One is a beautifully shaped but incomplete vase the upper part of which is adorned with a branch of a *pipal* tree. Another object is a handy combination of three copper instruments soldered together by their looped ends. They are a sharp-pointed awl, a double-edged knife and a pair of pincers possibly intended for surgical instruments.

Stratigraphy at Mohenjo Daro

H.T. LAMBRICK

The validity of an archaeological appreciation depends almost entirely on the quality of two processes applied to its object. It requires accurate observation, methodically recorded; and the data thus elicited should be assessed and interpreted in the light of genuine comprehensive understanding of the terrain—the climatic and other physical characteristics which must have governed its prehistory. Equal competence in both capacities is not attained by every archaeologist.

The work of the pioneer investigators of the Harappan site of Mohenjo-Daro, Sir John Marshall and Doctor Ernest Mackay, has been arraigned by their successors on the score of grave deficiencies in the procedure first mentioned—in that their excavation technique was bad and their method of record still worse (Wheeler 1954:18-22, 51-6, 127-8). The system of reference for purposes of record which Marshall applied to the seven strata of remains differentiated by him may be recapitulated in the words of Professor Stuart Piggott: "an extraordinary notation in distinguishing the successive phases (true stratigraphy was not recorded). There are three main 'periods', Early, Intermediate, and Late, each divided into three but with the sub-divisions numbered in *reverse* order, so that 'Early Period Phase I' is followed by 'Intermediate Period Phase III'." Everyone nowadays must agree with Professor Piggott's dismissal of "this impossible system" (Piggott 1950:139). It may be noted by the way that though the intention was to subdivide the Early Period it actually remained undivided throughout Mackay's time. More important is Mackay's statement that though he had intended "for convenience sake" to deal with his deep excavation in the G section of the DK area of the site in accordance with Marshall's three Periods and their sub-divisions he felt obliged to propose modifications in the light of evidence revealed by that excavation. Thus he declared "The so-called Late III phase should be regarded as the uppermost stratum of the Intermediate period" and "the Intermediate III phase should really be regarded as the last phase of the Early period" (Mackay 1938b: xiv). What the evidence was, which impelled him to revise Marshall's enumeration, will appear later. It should meanwhile be mentioned that the modified nomenclature does not seem to have been employed when objects found in those particular strata had to be recorded.

In 1948 Professor Piggott, in an article dealing primarily with the significance of certain peculiar artefacts previously recovered at Mohenjo Daro, took occasion to make three complaints against Mackay's record of his excavation of the area concerned in the DK mound.

(1) He had not produced a stratified section drawing.

(2) He had assigned objects "to their respective horizons by means of levels beneath an arbitrary datum rather than by reference to the natural layer of soil or debris in which they occurred."

(3) "A further complication" was his nomenclature of the building phases—"This extraordinary system of reference can be very confusing"—indeed the whole account of the Mohenjo Daro stratigraphy was "complex and sometimes inconsistent."

Piggott therefore produced his own "reconstruction" which, he claimed, though only an approximation, gave "in convenient visual form a rough outline of the probable sequence encountered by the excavator in the central area of the DK mound." To this rough sectional diagram he invites particular

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attention because "An important feature in the sequence is the presence of three thick layers of river-silt at various levels, indicating a flooding from the Indus, and of these three can be inserted on the diagram from Mackay's data" (Piggott 1948:27-9).

Mackay's own account of his deep excavation in the DK area appears on pages 42 to 44 of "Further Excavations at Mohenjo Daro," while particular features observed in its progress are mentioned also on pages xiv, xv, 2, 101, and 107-8. I cannot find that Mackay encountered more than one deposit of flood silt in this deep excavation in the DK mound; he describes it on page 44 as follows: "from 35 feet (Sc., below datum) downwards a layer of stiff clay with occasional pockets of grey sand is clear evidence of the occurrence of a flood." The other layers of silt recorded by Mackay were actually exposed in trenches he had dug in the flood-plain outside the inhabited area; though he did *correlate* two of these—erroneously, in my opinion—with the two levels of subsidence of buildings observed by him within the mound, which he held to have resulted from flood seepage (Mackay 1938b: xvi, 2, 3 and Marshall 1931:265).[1]

Piggott next remarks, with reference to the Marshall/Mackay nomenclature of the building phases, "The fact that flood silt cuts across these main periods in each instance was not apparently regarded as an inconsistency by Mackay." But it is not a "fact" that Mackay found flood silt "cutting across" these main periods in the deep excavation in the DK mound or, so far as I can ascertain, anywhere else within the inhabited area; though, as mentioned above, deposit of this kind was present in the lower part of the undivided Early Period down to (and presumably underneath) the sub-soil water level. As to inconsistency, it was on account of the position of widespread *subsidences* at two widely separated levels within the DK area (*not* strata of flood silt) that Mackay proposed some modification of the stratigraphy derived from Marshall. He ascribed these subsidences to two exceptional floods; thus "the so-called Late III Phase should be regarded as the uppermost stratum of the Intermediate phase . . . terminated by a complete evacuation of the city on the occasion of a great flood. . . . It is the Late II phase that marks the real beginning of the Late Period." Similarly "the Intermediate III Phase should really be regarded as the last phase of the Early Period. It was brought to an end by the earlier of the two floods of which we have evidence" (Mackay 1938b: xiv, xvi). Piggott's "reconstruction" of these lower levels reads "Virgin soil was not reached in the DK area, owing to the present water table, but flood silt was encountered, above which two imperfectly distinguished building levels lay, themselves covered by the silt of the first recognizable flooding of the city. Above this were two more building phases, Early I and Intermediate III before the second layer of flood silt" (Piggott 1948:27).

Piggott's "reconstruction" thus appears completely to misrepresent Mackay's actual findings; and since there is in reality no difficulty in delineating in section the latter's stratification as revised by himself, and the substances and indications which he did find in his deep excavation in the DK mound and in separate trenches dug in the flood-plain, with the approximate positions of these trenches; or in showing the strata throughout in accordance with the references to datum given by Mackay himself. I have drawn all these in the accompanying sketch (Fig. 23.1), inserting also Professor Piggott's "reconstructed" section at (I trust) its correct level, for purposes of comparison. And in the hope of preventing renewed misunderstanding I append explanatory notes to this diagram.

On the right-hand side of the sketch appears a schematic representation of part of the DK mound in section both above and below the modern level of the flood-plain, showing some substances encountered by Mackay in the southern portion of the G-Section of the DK area, where he carried out his deep digging. The datum-level at 178.8 feet above sea level which he employed for vertical references is drawn across with a scale on the right in feet indicating depths below datum, as given by Mackay throughout his text.

The stratification by periods and phases in my representation of the mound in section is that set forth on pages xiv to xv of Mackay's Introduction to "Further Excavations" embodying the modifications required by his remarks, viz., "The so-called Late III Phase . . ." appears as "the uppermost stratum of the Intermediate Period" (Intermediate I), with "the Intermediate III Phase . . . regarded as the last phase of the Early Period" (Early I).

I have also drawn the *mean* level of the 1927-31 A.D. surface of the surrounding plain at 157 feet above sea level (21.8 below datum). This level is produced across to the left of the sketch, where sections of the six readily distinguishable trenches, etc., mentioned by Mackay as dug at different places in the surrounding plain, are shown side by side in section, with reference to his

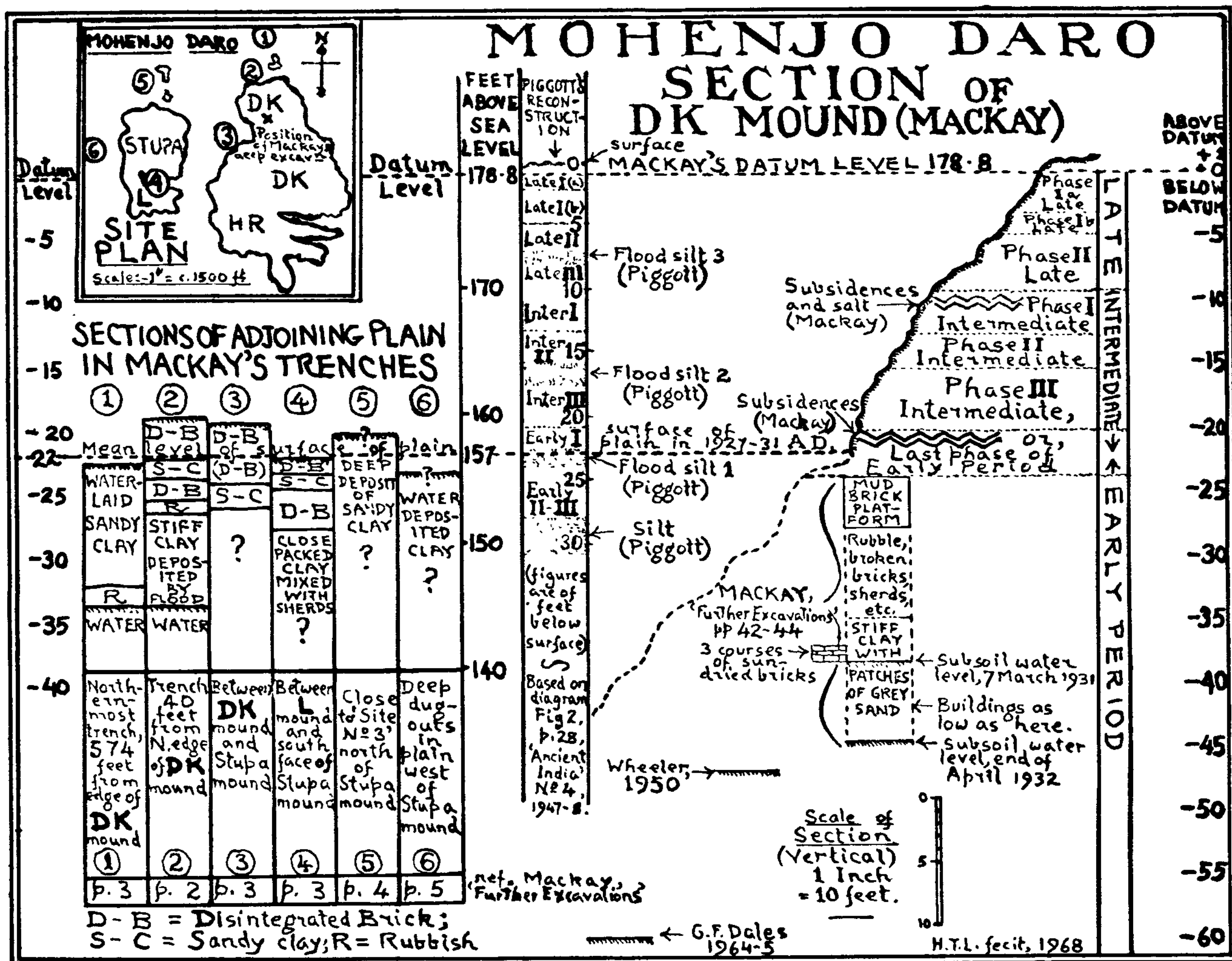


FIGURE 23.1. *The stratigraphy at Mohenjo daro.*

datum-level and details as in his text. Above these I have inserted a small plan of Mohenjo Daro, adopting the 160-foot contour line for the boundary of the mounds, so that the approximate location of the trenches can be conveniently seen.

Between the section of the DK mound and the sections of trenches in the flood-plain I have inserted Professor Piggott's "reconstruction" diagram as given in Fig. 2 of his 1948 article in *Ancient India*, No. 4, omitting his representation of the Axe adze and pins, but including his scale of feet below surface. I have taken his "surface" (=0) to correspond with Mackay's highest level of Late Phase Ia in the DK mound, viz., .86 above datum (Mackay 1938b: xiv).

The general levels of those subsidences in the DK area which Mackay ascribed to the effects of floods (percolation of the mound) have been inserted just below, i.e., prior to, phases Intermediate II and Late II respectively; this being the obvious interpretation of his remarks on pages xiv, 2, 8, 101, 107-8 of *Further Excavations at Mohenjo daro*, and at page 265 in Marshall's (1931) Vol. I, and Plate LXII in Vol. II.

The stratification of Mackay's deep excavation within the mound has also been inserted by reference to the details he gives on pp. 42-44 of *Further Excavations*. Included is the only instance that I have been able to find in his account of his investigation within the DK mound of flood deposit actually encountered and recognized as such—"from 35 feet (Sc., below datum) downward a layer of stiff clay with occasional pockets of grey sand is clear evidence of the occurrence of a flood." The deepest points reached in 1931 and

1932 are also shown, and for ready reference and convenience I have added the levels reached by Wheeler in 1950 and by G.F. Dales in 1964-5 as if they too had been excavated through the DK mound.

The strata of deposits of various substances observed by Mackay in the trenches or other excavations *in the surrounding plain* have been drawn in accordance with the data he gives on pages 2 to 6 of *Further Excavations* and two of them, numbered by me 1 and 2, checked with the diagrams in section which figure in his Plate VIII, Figs. H. 8 and H. 2 respectively.[2]

I trust that these and the other details and levels mentioned by Mackay have been correctly indicated in the sketch, as also those which appear in Professor Piggott's diagram of his "reconstruction." It is surely easy enough to relate feet above sea level to Mackay's feet below datum, and with Piggott's feet below surface (i.e., of the DK Mound).

Unless I have fallen into some strange error, there is no correspondence between the positions of Piggott's "layers of flood-silt" and Mackay's levels of observed subsidences and of the deposit of "stiff clay with pockets of grey sand" interpreted by him as evidence of floods. Piggott's "Flood Silt Three" lies higher than the subsidences accompanied by salt which Mackay attributed to a flood at the end of the Intermediate Period: while the reconstructor's "Flood Silt Two" occurs above, and his "Flood Silt One" below, the level of the earlier subsidences thought by Mackay to be due to a flood which closed the Early Period. Again Piggott's indication of "Silt" about his thirty-foot level (below surface) lies several feet above the deposit of "stiff clay with pockets of grey sand," the upper surface of which Mackay states occurred at thirty-five feet below datum in the DK mound.

On the other hand, the levels of Piggott's "Flood Silt One" and of his basic "Silt" do quite closely correspond with those of the two layers of sandy clay in Mackay's trench No. 2—dug in the plain forty feet away from the northern edge of the DK mound.

Now if these details and comparisons have been fairly and accurately set forth above, the curious coincidence last mentioned only reinforces the impression left by the other extraordinary discrepancies, viz., that in this instance the work of Doctor Ernest Mackay has been most grievously misrepresented and traduced, apparently through inadvertence. And the mischief extends further than injustice to Mackay. Professor Piggott's diagram seems to have become accepted by other writers on this subject, during some twenty years following its publication, as if it were the most authentic possible exposition of the stratification of the "Lower City"—something by virtue of which they were exempted from the task of examining for themselves Mackay's own statements and diagrams connected with his excavations.

Thus on page 119 in the third edition (1968) of *The Indus Civilization* we find Sir Mortimer Wheeler writing of "deep flood deposits such as the *three observed by Mackay amongst the higher levels* penetrated by him" (my italics, H.T.L.); and again on page 127, "It would appear *from the records of the principal excavators,*" (again my italics) "(who unhappily recorded their observations with baffling inadequacy) that at Mohenjo Daro periods of occupation were interleaved by three main phases of deep flooding." To the first of these statements a footnote is appended "See discussion of Mackay's alleged stratification by Piggott in *Ancient India* No. 4 (1948), pp. 27 ff."; and to the second "Reconstructed *so far as the evidence permits*" (my italics once more) "by Piggott in *Ancient India* No. 4 (1948), p. 28." And that article is cited as authority in support of similar assumptions on page 55 (Wheeler 1968:55, 119, 127).

Prior to the appearance of this edition of Wheeler's work Mr. R.L. Raikes had adduced Piggott's (factitious) "thick beds of river silt" as significant data in the evolution of his theory of deep still-water flooding of Mohenjo Daro. In his 1965 article *The Mohenjo Daro Floods*, Raikes alludes to his earlier paper *The End of the Ancient Cities of the Indus* (Raikes 1964) observing, "Reference was made to the only existing attempt (by Piggott) to reconstruct the stratigraphy of earlier excavations. The most important arguments were the great thickness of individual silt beds indicated *in the reconstructed stratigraphy*" (my italics, H.T.L.). . . . "It was clear that Marshall's thirty feet or more of deposits were all under the present flood plain but it was not known whether Piggott's stratigraphy referred to the same zone. If the archaeological evidence was right all this deposition of silt took place only during the occupation of the city." But in the sequel, when Mr. Raikes obtained the opportunity of carrying out some investigations at Mohenjo Daro he found—not surprisingly—that "analysis of the actual levels of flood deposits does not indicate the existence of general inundation levels as suggested in Piggott's stratigraphy" (Raikes 1965c:196-7). Instead, what he deemed to be still-water flood deposits appeared

almost omnipresent in the ruins up to twenty-nine feet above the level of the modern plain.

I have explained elsewhere what I believe to be the origin of these deposits (Lambrick 1967a). It is unnecessary to recapitulate in this place the arguments adduced by me in 1967 against the great tectonic uplift-lake-submergence theory for the decline and fall of Mohenjo Daro; I would merely repeat that the above-mentioned numerous sporadic layers of alleged still-water "Silty Clay" etc., require objective examination and interpretation by a qualified sedimentologist before they can properly be accepted as evidence of lacustrine as distinct from aerial deposition.

Meanwhile the archaeological reading public, accustomed for the last twenty years and more to hear the work of Marshall and Mackay disparaged on the ground that their methods produced confusion, may well regret that efforts purporting to clarify it should leave confusion worse confounded.

NOTES

[1]I calculate that the inundations which would have contributed to form those distinct strata of sandy clay exposed in Mackay's trenches in the plain numbered by me 2 and 4, must have occurred well within the Christian Era, several centuries apart. The whole subject is examined at length in my *Sind Before the Muslum Conquest* (Lambrick 1973).

[2]In his account of the trench designated by me No. 3, Mackay seems by a slip of the pen to have wntten "South-East" in place of "South-West" (Mackay 1938b:3, cf. footnote same page).

New Investigations at Mohenjo Daro

GEORGE F. DALES

Mohenjo-daro, capital of the ancient Indus civilization (also known as the Harappan, from the site of Harappa in the Punjab), was extensively excavated during the 1920s and early 1930s. Almost one-third of the area of this huge city was exposed, revealing for the first time the remains of the oldest civilization of South Asia. But little incontrovertible information was gathered concerning the origin, life history and decline of the city and the civilization it represents. The lowest and earliest levels are many feet below the present ground water level of the area. Past attempts to penetrate to these lowest levels failed because it was impossible to remove the water. One of the mysteries which we hope to solve is the presence of numerous deposits that seem to have resulted from floods. These are found at Mohenjo-daro and other sites in the lower Indus Valley. The highest recognizable flood deposits at Mohenjo-daro are thirty-one feet above the present level of the plain!

The original excavations at the site were directed by Sir John Marshall and were published under the title *Mohenjo-daro and the Indus Civilization* (1931). Excavations were continued (1927-1931) by E.J.H. Mackay, who published *Further Excavations at Mohenjo-daro* (1938b). Sir Mortimer Wheeler made additional investigations there in 1950 but did not make a formal publication of the results, only referring to them in his books on the Indus civilization.

The University Museum of Philadelphia and the Pakistan Department of Archaeology have completed the first season of new excavations at Mohenjo-daro. These are one facet of an overall project to study and re-evaluate the ancient geography and ecology of the lower Indus Valley. Geological and archaeohydrological studies are being conducted by our engineering consultant, Robert L. Raikes.

The first phase of the initial season's work involved the drilling of exploratory holes to determine the maximum depth of occupation. Three drillings were made near the area of our proposed new excavations by the Indus Valley Construction Company of Lahore. Samples of soil and artifacts were collected from levels at about two-foot intervals down to virgin soil. The results of two borings at the base of HR mound (see Fig. 24.1 and Fig. B) were identical, showing that occupation in this particular area of the site starts thirty-nine feet below the present level of the plain. The mound itself in this area is about thirty-five feet high, and thus the total depth of occupation is about seventy-four feet. As the present ground-water level is only some fifteen feet below plain level, it will thus be necessary to penetrate at least twenty-four feet below the water table in order to reach the earliest level of occupation. Raikes, in conjunction with several Pakistan engineers and government officials, is designing a dewatering system adequate to permit the deep excavations planned for the third season of the project.

Digging has thus far been concentrated along the western edge of the so-called HR area. This location was selected because it was one of the highest unexcavated areas and could yield a complete occupational sequence, and because at the same time we could investigate the enigmatic north-south gully that separates the city into two distinct parts.

An area approximately 20 × 20 meters was opened on top of the HR mound. It is impossible to determine exactly how much of the later material has been lost through erosion and exposure during the past thirty-

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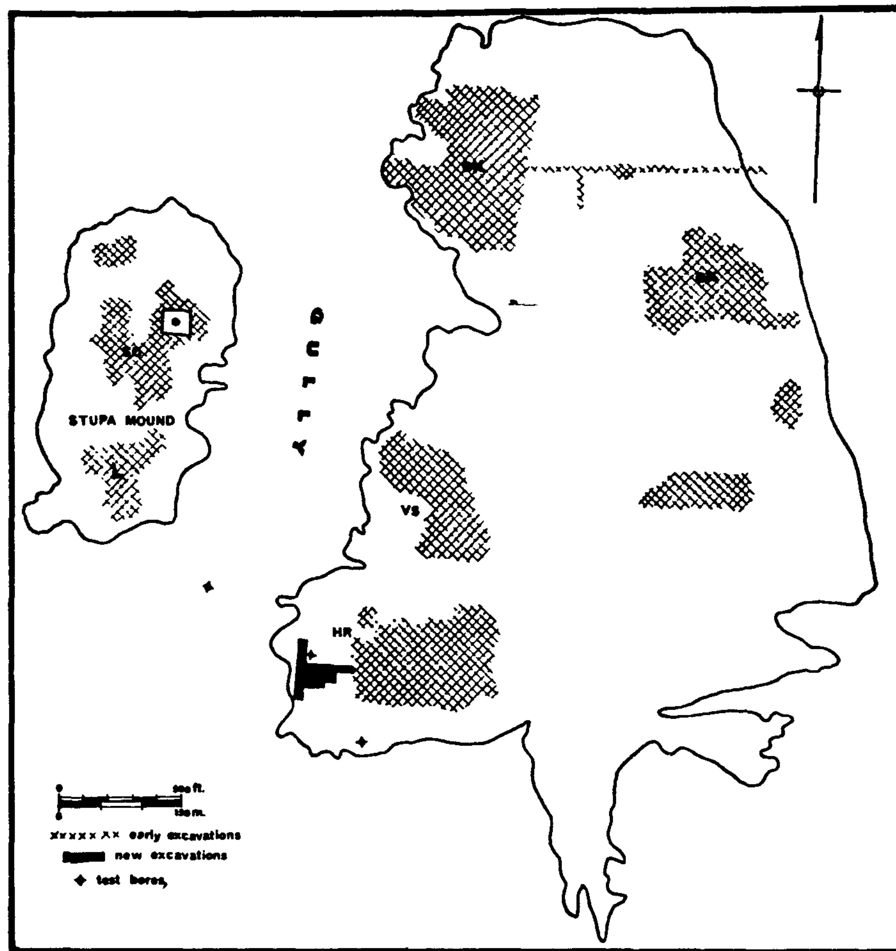


FIGURE 24.1. *Plan of Mohenjo daro showing areas of excavation*

five hundred years. The exact date of the Harappan civilization is still in doubt but it is thought that the "mature phase" extended from 2500 to 1700 B.C. No traces of post-Harappan occupation were found; abundant evidence exists, however, for what is almost certainly the final phase of the Harappan occupation. Our uppermost level follows the pattern indicated by the earlier excavations at the site and most recently by the French excavations at Amri, about ninety miles south of Mohenjo-daro. On this level were jerry-built houses made with used, often broken, bricks and other indications of a squatter type of occupation rather than just a late phase of the Harappan civilization. What few finds could be definitely associated with these latest structures were basically of Harappan type but of a simple and degenerate nature. We may provisionally call this the Squatter Period, to distinguish it from the Late Period described by the previous excavators. Late Period remains were directly beneath the thin Squatter level and in places were even exposed on the mound surface. There were suggestions that some of the rooms had been hastily abandoned. Cooking pots were found on one of the floors, while another room had been severely burned. A thick layer of straw and chaff had smoldered on the floor after the roof collapsed. Most important was the discovery of burned wood door jambs, still in place in a doorway leading from this room.

The most startling find in the upper levels of the HR mound was made on the second day of excavation. Between walls bordering a narrow alleyway was found a very thick accumulation of collapsed brick, ash and broken pottery. Enmeshed in this debris were found five skeletons. They resemble some thirty others found earlier at this site—not buried in any normal way but the victims of some disaster.

Typical Indus seals of steatite, depicting animals and signs in the still undeciphered Indus script, were found in these Late Period levels. Small square seals of glass paste with geometric designs—including the swastika—were found only in the Late Period levels of our excavations. Certain ceramic forms and styles of painted decoration can be assigned to this period. This helps to refute the widely accepted view that the civilization was a static, uninspired monument of ultra-conservatism. The animal figurines from the late levels are of a simple toy-like variety with little or no modeling of the body contours. Facial details are depicted by deep holes and carelessly incised lines. The unnaturally large eyes are usually represented by pierced clay discs applied to the head. These figurines contrast sharply with the beautifully modeled, carefully detailed animal figurines from our excavations at the base of the HR mound. The bull figurines, probably belonging to the most flourishing period of the city's life, were probably of composite construction. The bodies and legs were carefully modeled by hand while the heads—especially the faces—appear to have been made in molds. Horns and ears were attached separately and tiny clay pellets were inserted as eye-balls. Although there are at least two distinct styles of animals, all have one feature in common—with scarcely an exception their heads are turned to the right. Among over two hundred animal figurines not a single representation of a cow was found.

Excavations on the edge of the mound and at its base revealed unexpected and highly suggestive results, but since the dating of the lower levels and structures is still uncertain, we cannot say more at the present time. At the edge of the mound is a massive construction composed mainly of huge solid mud brick embankments with baked brick retaining walls. At one point an exploratory pit was dug into an embankment, but digging was stopped at a depth of twenty-five feet without reaching the bottom of it.

At the level of the plain a series of solid baked brick walls at least twenty feet high was found running north and south. These seem to provide the facing for the mud brick embankments which form the edge of the mound. It is premature to identify these monumental structures positively, but it is tempting to consider the walls and embankments as part of a city wall surrounding at least the eastern part of the city. Sir Mortimer Wheeler reported the discovery in 1950 of a wall associated with the citadel or stupa mound in the western part of the city. The possibility thus arises that the citadel and the habitation area of the city each had its enclosure wall and that there was no single wall surrounding the entire city. This would fit in with the possibility that the north-south depression separating the eastern and western parts of the city was originally a canal or a branch of the river. The massiveness of the walls and embankments suggests a function beyond that of a simple defence wall.

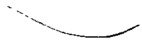
If the theory proposed by Raikes is fully substantiated, the building of such structures would have been a logical move on the part of the Harappans to defend their city against the slowly rising flood waters. Before presenting the results of the flood studies to date, one other architectural feature should be mentioned. Considerable evidence was found in the excavations at the base of the mound for the combined use of baked brick and wooden architecture. Wooden beam sockets, recesses in brick wall faces for wooden beams, and a series of regularly spaced vertical slots on the outer wall surface of one building point to the use of wooden architectural components on a scale much more extensive than had hitherto been realized.

The flood studies included, first of all, the examination, leveling and recording of over 150 flood deposits at Mohenjo-daro, most of which had been exposed during the earlier excavations. Examinations were made at other sites in the valley, including Amri, Lohumjo-daro, Jhukar and Sewan, and intensive studies were made of geological and geographical records.

Tentatively, the evidence suggests that Mohenjo-daro and much of the lower Indus Valley suffered from a series of severe and extensive floods which eventually forced most of the population to abandon the area, possibly around 1800 or 1700 B.C. The flooding, which apparently occurred several times during the centuries, resulted from earth movements, some of which may have taken place in the vicinity of modern Sewan. These tectonic uplifts created a dam which retarded the flow of the Indus and formed a sort of reservoir which may have extended as far north as modern Sukkur. The flood waters would have risen slowly enough to enable the people of Mohenjo-daro to construct the massive embankments mentioned above. The mechanics involved in this suggested series of tectonic uplifts and subsequent downcuttings of the Indus plain are highly complex and require consider-

able study. The identification of flood deposits at other sites in the valley must be undertaken in conjunction with a program of exploratory drillings and soundings.

We are confronted here with problems which require the close cooperation of the archaeologist and the natural scientist. At the close of the first year's work significant advances have already been made toward a better understanding of the life and death of one of the world's great early civilizations.



Kalibangan : A Harappan Metropolis beyond the Indus Valley

B.K. THAPAR

Kalibangan, literally black bangles, from the sight of the countless fragments of weather-stained terracotta bangles strewn over the surface of the site, lies some 310 kilometers northwest of Delhi, along the left bank of the now-dry river Ghaggar in the northern part of Rajasthan. Anciently, the river, often identified with Sarasvati, reached to its Hakra branch in Bahawalpur, Pakistan. Although some part of this valley had been explored in 1940-41 (Stein 1942), it was in only ten years thereafter (Ghosh 1952) that as many as twenty-five Harappan sites were identified within the present-day borders of India in the "region beginning right from the Pakistan border (eastwards) up to midway between Hanumangarh and Suratgarh in the Sarasvati Valley and about 22 kilometers east of Bhadra in the Drishadvati Valley."

Among these newly-discovered sites, Kalibangan appeared to be of potential importance. It comprised two mounds of moderate size, with the smaller one (KLB-1) located to the west and the larger (KLB-2) to the east, recalling identical disposition of mounds at Mohenjo-daro and Harappa, and thus holding out possibilities of adding substantially to our knowledge of the cultural style of the Indus Civilization beyond the Indus Valley. The next decade or so was a period of continuing explorations and discovery in Gujarat. Planned excavations at Kalibangan could be undertaken only from 1961 onwards and lasted for nine field-seasons ending with 1969 (*Indian Archaeology, 1961 to 1969—A Review*). Among other things, the excavation brought to light the grid layout of a Harappan metropolis, perhaps truly the "first city" of the Indian cultural heritage. The significant part of the evidence, however, relates to the discovery of a non-Harappan settlement immediately underlying the occupational remains of the Harappan citadel (KLB-1). Kalibangan thus became the fourth site, after Amri, Harappa and Kot Diji, all in Pakistan, where the existence of preceding cultures below that of the Harappan has been recognized. Subsequent excavations in Pakistan added yet another site of this class at Gumla in the Gomal Valley (Dani 1970/71). The present paper gives a resume of the evidence as obtained at Kalibangan (Lal and Thapar 1967, Thapar 1973a and 1973b) in the order of the cultural sequence.

Before proceeding with the evidence it would perhaps be desirable to explain the terminology adopted here for the two cultural periods, Harappan for the upper one and pre-Harappan or antecedent for the lower. While the former culture label is widely accepted, it is the latter which has given rise to dispute. The deposits of this culture have variously been designated as proto-Harappan, Early Harappan, pre-Harappan and Early Indus (Thapar 1965c, 1973a and 1973b; Dales 1973; Moghul 1970). Each of these labels has its own inherent drawbacks and cannot be used as a blanket term for all cultures immediately preceding that of the Harappan in different regions. A detailed analysis of the question which is intimately connected with the actual genesis of the metropolitan culture would involve considerable discussion and is not being attempted here. Suffice it to mention that in the present stage of research it would be appropriate to adopt the site-names (fully spelled) with the cultural period shown in Roman numerals. Thus, for Kalibangan the earlier period is called Kalibangan I and the later Kalibangan II. This, however, should not be confused with KLB-1 and KLB-2 which stand for names of mounds—KLB being an abbreviation for Kalibangan,

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and Arabic numerals being used for numeration of sites (Fig. D).

KALIBANGAN I (PRE-HARAPPAN)

The settlement was situated on the bend of the river beyond the active flood-plain and was a parallelogram, some 250 m. from north to south and 180 m. from east to west. It was found to have been fortified from the very beginning of the occupation. The fortification wall was made of mud bricks (30 × 20 × 10 cm.; proportion, 3:2:1) and in its extant portion showed two structural phases. In the earlier phase, the basal width was 1.9 m. while in the latter, it measured 3 to 4 m., the extra thickness being added on the inner side. Both the inner and outer faces of the wall seem to have been originally plastered with mud, patches of which were found preserved at many places.

Within the walled area, the houses were built of mud bricks of the same size as those used in the fortification wall, the masonry being in the English bonding technique, with alternate courses of headers and stretchers (Plate XI). The use of baked brick was attested by a drain, the size of the brick being the same as that of mud bricks. In the limited area of the excavation (largely on the slope of the mound) it was not possible to expose complete house plans. Nor was it feasible to dig at other places, for it would have involved removal of the superimposed Harappan deposits. Nevertheless, it was apparent that a house consisted of three to four rooms with a courtyard.

The excavation also brought to light part of a 1.5 m.-wide lane running in the east-west direction. From the width of the walls (some of them only a single brick length) it could be inferred that the houses were single storeyed (Plate XII). Interesting evidence regarding cooking practices was revealed by the presence, inside the houses, of ovens both of the underground and above-ground variety, closely resembling the present-day *tandoors* in the region. The former had mud-plastered walls with a slight over-hang near the mouth and the latter, also made of mud walls with bridged side opening for feeding fuel, seem to have been periodically plastered. Equally noteworthy was the existence of cylindrical pits lined with lime plaster, possibly for storing drinking water. The alignment of the houses no less than the size of the bricks used was significantly different from those of the succeeding Harappans.

The distinctive trait of this Period, however, was the pottery (Thapar 1969a) which was characterized by six fabrics, labelled A, B, C, D, E and F (Plate XIII). Of these, fabrics E and F, distinguished essentially by surface color (E for buff and F for grey), did not show marked individualities either in shape or in painted design, their main features being shared by other fabrics. Furthermore, in frequency they were somewhat uncommon, the latter being particularly scarce.

Fabric A was marked by an individuality which isolated it from the other fabrics. This was the most commonly used pottery. The vessels of this fabric, although made on the wheel, were carelessly potted, showing unskilled handling with tell-tale traces of irregular striations. It was drab red to pinkish in surface color and painted over in light black, combined at times with white, the field of decoration being confined to the portion above the girth. The design elements, drawn in free style, included horizontal bands, loops fringed below or enclosed by horizontal bands, a criss-cross moustache-like bifold scroll within wavy lines, symmetrically joined semicircles, latticed triangles, lenticulars, segments or scallops with figures. The range of shape was, however, very limited and comprised vases with outturned or outcurved rims and disc or ring bases, and bowls with tapering or convex sides. Of unusual interest were a vase with a pedestal base and another with a hole mouth.

Fabric B was distinguished primarily by its paste, texture and surface treatment. The pottery of this fabric was carefully potted on the wheel and was treated with a red slip up to the shoulder, the slipped area being further diversified by black painted designs. The remaining surface of the pot was covered by a thin clayey solution and, while wet, seems to have been roughened by horizontal or wavy combings or by tortoise shell or dendritic impressions. Over this rusticated surface, naturalistic designs—floral, animal and bird—were painted in black. Only one shape, the jar, was represented in the fabric. The surface elaboration of this fabric closely resembles that of the so-called "wet wares" including Khojak Parallel Striated of the Quetta region.

Fabric C was marked by a fine-textured paste and all-over smooth-slipped surface in shades of red and purple or plum red, the latter recalling pottery from below the defensive walls at Harappa. The repertory of painted designs included, besides the recurrent carefully ruled horizontal bands or loops or criss-cross, borders of plants, fish-scale, metopes, pendent triangles, etc. The shapes represented in this fabric

comprised globular and ovoid vases with disc bases, lids, straight-sided bowls, dishes and offering stands. Except for a few diagnostic shapes, this fabric was the nearest in correspondence to the Harappan pottery.

Fabric D (Plate XIV) was characterized by a thick sturdy section and a slipped red surface. Common shapes included heavy jars, troughs and basins. The last named was, however, the most characteristic type. The basin interiors were decorated on the sides with sharp ridged incisions of varying pattern and on the outside with single or multiple rows of cord impressions. The shape and design of this basin are closely paralleled at Amri in Period II B.

Among the other finds of this Period, the more noteworthy were: small-sized blades of chalcedony and agate, sometimes serrated or backed; beads, variously of steatite (disc), shell, carnelian, terracotta and copper; bangles of copper, shell and terracotta; terracotta objects like a toy cart-wheel and a bull; quern stones with mullers; a bone point; and copper celts including a curious axe (Plate XV).

An outstanding discovery of the excavation, however, was a ploughed field situated to the southeast of the settlement, outside the town wall. This is perhaps the earliest plough-field excavated so far. It showed a grid of furrows with one set more closely spaced (about 30 cm. apart) running east-west and the other, widely spaced (about 1.90 m. apart), running north-south. Of these, the former seemed to have been ploughed first. Curiously enough this pattern bears a remarkable resemblance to modern ploughing in the neighborhood, wherein two types of crops (pulse in one direction and mustard in the other) are grown in the same field, the combination being dependent upon the size and growth behavior of the plants. No remains of either a plough or ploughshare or coulter have, so far, been obtained from the excavation. The material of which the ploughshare, etc., was made, as also its shape, therefore still remains to be established. Since cultivation during this Period seems to have depended on flood-irrigation, supplemented by seasonal precipitation, it is reasonable to infer that only the winter crop (*rabi*) was grown, the sowing being done in the autumn after the river flood resulting from the tropical monsoon had subsided. Although no cereals were found in the course of excavation, cereal-type pollen has been attested in good numbers in the deposits of Period I (Singh *et al.* 1974). The evidence points to an emergent stage, though in a fashion which could promote further transformation. The people were capable of dealing with the demands of the river.

The occupation endured through five structural phases, rising to a height of 1.6 m. above the natural soil, when it was brought to a close by a catastrophe (perhaps seismic), as evidenced by the occurrence of displaced (faulted ?) strata and subsided walls in different parts of the excavated area. Thereafter, the site seems to have been abandoned, though only temporarily, and a thin layer of sand, largely infertile and wind blown, accumulated over the ruins. During this period, the peripheral portions of the mound, particularly on the east and the west, seem to have been badly eroded and gullied.

Now to chronology: The stratigraphical position of the settlement of Period I clearly establishes its priority in time-range to that of the succeeding Period which represented the Harappa Culture. The chronological bracket of the latter (2500-1750 B.C. at Mohenjo-daro) cannot be applied mechanically to Indus towns and villages in all locations. Many of the settlements, particularly those in the Ghaggar Valley, may have been founded later than the nuclear cities of Mohenjo-daro, Harappa, etc. In the light of the available evidence, it may be postulated that the Harappan occupation at Kalibangan would have begun around 2300 B.C. and lasted up to 1750 B.C.

Turning to absolute chronology, nine samples (five from the Early, one from the Middle, and three from the Late levels of Period I) have been dated by radiocarbon determination. Except for one, the dates are all consistent and indicate an inclusive time range of 2450-1900 B.C. which suggests an apparent overlap between the two cultural Periods. No such phenomenon, however, was recognized by the excavations, except that in the lower levels of the Harappan occupation, especially in the lower city area, pottery of Period I assemblage occurred alongside of the Harappan. This suggests only that the old inhabitants remained in contact with the new arrivals. On the other hand, there is conclusive evidence, as previously stated, to suggest a break in the occupation of the site after the close of Period I. As the excavated cuttings from which the samples have been obtained for radiocarbon dating lie on the slopes of the mound with very little soil cover (Agrawal and Kusumgar 1968b), the dangers of humus contamination must be kept in mind, especially in respect to samples belonging to the Late levels of the Period. The duration of Period I, therefore, would in fact be much shorter, and on the above showing is estimated as 2450-2300 B.C., with a margin of some

years for the abandonment of the site at the upper end of the scale. The five structural phases, of which the upper three were largely rebuildings (without changes of plan), would fully support this postulate.

On the basis of the comparison of the material equipment of Kalibangan Period I with that of corresponding cultures at Harappa, Amri and Kot Diji, it is seen that while these village town cultures share a common level of economic subsistence, they are marked by regionalization with an uneven development and essentially differing ceramic traditions. However, one of the characteristic types of Fabric D of Kalibangan I is found to be closely paralleled in Amri II B (Casal 1964a), thus providing a datable correlation between the two cultures on a common time scale. On the strength of a radiocarbon date, Amri IC is dated to *circa* 2600 B.C. Amri IIB, therefore, would fall around 2450 B.C., which agrees with the date proposed above for the beginning of Kalibangan I. Confirmatory evidence is also forthcoming from layer 5 at Kot Diji, representing the late phase of the Kot Diji Culture and dated to *circa* 2300 B.C. Another noteworthy analogue in both form and fabric is provided by the globular vase with short neck and externally grooved body, found at Kalibangan I (Fabric C), Sarai Khola II, Pre-Defense Harappa and Kot Diji.

The correlation of the above-mentioned assemblages would indicate that the so-called pre-Harappan communities appeared in northern Rajasthan somewhat later than on sites in Baluchistan and the Indus Valley proper, as though reflecting a "sloping horizon" of cultural level from west to east.

The dates proposed above do not take into consideration the correction factors proposed by the physicists for radiocarbon dates. According to these calculations, 500 to 550 years are to be added to the standard radiocarbon dates.

KALIBANGAN II (HARAPPAN)

In Period II the structural pattern of the settlement was changed. There were now two distinct parts: the citadel on the west, represented by a smaller mound (KLB-1); and the lower city towards the east, represented by a fairly extensive mound (KLB-2). The former was situated atop the remains of the preceding occupation (Period I) to gain an eminence over the lower city which was laid out on the natural plain towards the east, leaving a gap of over 40 m.

The citadel complex is roughly a parallelogram some 240 m. from north to south and 120 m. from east to west, and consists of two almost equal but separately patterned parts, rhomboid on plan. Both these parts were contained by a fortification wall 3 to 7 m. in width (on the southern sides, the width was extended to 9-11 m.) and reinforced at regular intervals with rectangular salients or bastions. The fortifications were built throughout of mud bricks; two sizes of bricks, 40 × 20 × 10 cm. and 30 × 15 × 7.5 cm. (ratio: 4:2:1), representing two principal structural phases, were used in the construction, the larger bricks in the earlier phase and the smaller in the later. On the north and the west the fortification wall overlies that of the preceding period, while on the east and the south, including the central portion (partition fortification wall), it was built on the ruins of the earlier occupation which was often dug into for foundation purposes. Both the inner and outer faces of the wall were originally plastered with mud, traces of which were available at a number of places.

The southern half of the citadel was more heavily fortified not only with corner bastions (Plate XVI) but also with rectangular salients along the northern and southern sides. While no complete plans of the corner bastions were available, the salients projected 8 to 9 m. from the main face, and were 13 to 17 m. broad. They rose imposingly with a battered face, the taper being obtained by the coating of plaster. The enclosed area contained some five to six massive platforms of mud bricks (40 × 20 × 10 cm. for the earlier phase and 30 × 15 × 7.5 cm. for the later), each separate from the other and intended to be used perhaps for a specific purpose by the community as a whole. Of these, sizable portions of five including the complete outline of one (approximately 50 × 25 m.) have so far been exposed. All these platforms were found to be oriented along cardinal directions. While in the case of the fully-recovered one the longer axis was east-west, for the remaining three it was north-south. The size of the platforms no less than the width of the passages separating them varied. At no point, however, were these platforms joined to or integral with the fortifications. Access to the working floor of the platforms was by means of steps which rose from the passage. At one place the passage fronting the platform was found to be paved. Through the passages also ran baked-brick drains. Of the buildings which stood upon these platforms, no intelligible plans are available as they had been obscured by depredations of brick robbers.

Nevertheless, the available remains do indicate that some of these might have been used for religious or ritual purposes (Plate XVII). While on the one with the known complete outline, besides a well and a fire altar, a rectangular pit (1.24 × 1 m.), lined with baked bricks and containing bones of a bovine and antlers, representing perhaps a sacrifice, were found (Plate XVIII), atop another was noticed a row of seven rectangular "fire altars" aligned beside a well.

The entrances to this part of the citadel were located on the south and north. The southern one was situated between the central salient and the southwestern corner tower and, by virtue of its being an impressive baked-brick structure, seems to have suffered very badly from modern spoliation. The passage floor or steps along with its baked-brick flanking masonry had been ransacked for bricks with the result that only the impressions of it were available during the excavation. The passage was 2.65 m. in width, and, in the absence of well-preserved structures, could be inferred to be stepped, fronting the main fortification wall. The northern entrance comprised a stairway which, running along the outer face of the partition fortification wall between the two centrally located salients, led up to the required height at which passage across the fortification wall was provided.

In this entrance complex again, two structural phases were recognized, of which the earlier consisted of steps built of mud bricks (40 × 20 × 10 cm.) with a riser of 10 cm. and a tread of 40, and the later, perhaps of a ramp, screened by a 1.5 m.-wide wall (30 × 15 × 7.5 cm.). Lest the unwary reader carry the impression that Kalibangan during Period II (Harappan), the small-sized bricks, viz, 30 × 15 × 7.5 cm., were introduced only in the later phases, it may be said that bricks of this size have been used right from the beginning of the occupation for domestic structures. It was only in the fortifications and massive platforms that the large-sized bricks were employed in the earlier phases and smaller ones in the later. It is significant, however, that the ratio of dimension of both sets remains 4:2:1. From the location of the entrances, it is surmised that the southern entrance may have been the main one intended for the general public from the lower city and the northern for the dwellers in the residential annex (northern half) of the citadel. The structural feature of both these entrances, however, precluded the possibility of any vehicular traffic within the enclosed area.

The northern half of the citadel, which was also fortified, contained residential buildings perhaps of the elite (Plate XIX). Although full details of the house plans could not be recovered, it was noticed that the houses were built away from the fortification walls of the citadel, perhaps to give some privacy to these structures. The passage between the partition wall and the houses was found to be paved with brick-on-edge, extending from the northwestern corner bastion (of the partition fortification wall) to the easterly of the two central salients and perhaps reaching to the entrance stairway. There was evidence to show that this pavement was renewed at least three times during the lifetime of the citadel. The size of the bricks used in all these floorings was 30 × 15 × 7.5 cm. A complete street plan of this part of the citadel has not been obtained (Plates XX, XXI and XXII).

Meanwhile, a thoroughfare running north-south has been partially explored to a length of 40 m. Starting from the easterly of the two salients of the partition fortification wall, it ran obliquely in the direction of the entrance on the north. There were three (one each on the eastern, northern and western sides) entrances to this part of the citadel, none of the ramp or stairway type. Inside the eastern one was the brick-on-edge pavement referred to above.

The lower city was also a parallelogram, some 240 m. from east to west and 360 m. from north to south, and lay to the east of the citadel beyond a broad space of some 40 m. It was found to be enclosed by a fortification wall ranging in width from 3.5 to 9 m. (involving three to four structural phases). It was made of mud bricks of similar sizes to those used for the fortifications of the citadel, viz, 40 × 20 × 10 cm. for the lower phase and 30 × 15 × 7.5 cm. for the upper. Within the walled city was a gridiron plan of streets running north to south and east to west, dividing the area into blocks.

The existence of four arterial thoroughfares running north to south and three (with an indication for the fourth in the northern part) running east to west was established by excavations. While the former were found to run unimpeded, the latter did not cross the former but were staggered on plan and possibly served as delivery or entrance lanes for certain house blocks. Again, while the former were not equally spaced, the latter were situated on an average of 70 m. from each other. Besides, there were quite a few other streets which served only one or two blocks and were not thoroughfares. The width of the thoroughfares and streets corresponded to the multiple of 1.8 m. and

ranged from 1.8 to 7.2 m. To avoid damage from the vehicular traffic, fender posts were provided at some street corners. The width of the thoroughfares seems to have been maintained throughout the occupation; the only structural encroachments into the streets were the rectangular platforms (of uncertain use) immediately outside some houses. The streets, except in the late phase, were unmetalled. No evidence of regular street drains has so far been encountered. House drains, which were either of wood or of baked brick, discharged into soakage jars buried under the street floor.

The existence of two entrances to the fortified area, one on the north and the other on the west, was attested by excavation. Of these, the western entrance, which was controlled by a guardroom, led to an east-west street (counting third from south upward) and the northern, located at the northwestern angle, communicated with the first and third (counting from left) thoroughfares which converged on this gateway. The other two north-south running streets did not open into any gateway but stopped over 5 m. short of the fortification wall, pointing to the existence of an open space between the ends of all the north-south thoroughfares and the gateway. From the situation of the two entrances, it could be inferred that the western one was used by the city dwellers for communicating with the citadel and the northern for the city's commercial traffic.

The layout plan of the city shows that the alignment of the streets is at variance with that of the fortification walls. While the precise reasons for this seeming deviation still remain to be ascertained it can be argued that the alignment of the fortifications was conditioned by the Period I (pre-Harappan) layout of the adjacent citadel and by later modification by the Harappans as occupation advanced. Excavation has revealed that the fortification and the streets had been planned at one and the same time, viz, the very beginning of Harappan occupation of the site. Interestingly enough, it was observed that the house walls near the fortification wall faithfully followed the alignment of the latter, while those nearer the streets followed the streets themselves.

From the very beginning of the occupation, the houses were built of mud bricks (30 × 15 × 7.5 cm.), the use of baked brick (of the same size and also the wedge-shaped type) being confined mostly to drains, wells, sills and bathing platforms. In the typical chessboard plan of the city, each house faced two, if not three, streets and consisted of a courtyard with six to seven rooms aligned on two or three sides. Entrance to the house, located usually on the lane side, was either through a courtyard or through a corridor running between sets of rooms. There was also evidence of the single leafed door or a stairway in some houses. Some of the courtyards contained a well, evidently used by two or three families. Some houses had also a "fire altar" in one of the rooms.

The finds obtained from the occupation of this Period were all characteristic of the Indus Civilization and need not be listed individually. Among these, the seals and the weights and measures in a graduated series of size testify to the high level of sophistication and standardization reached by the Indus Civilization. The following finds deserve special mention: a cylinder seal; a terracotta cake, incised on the obverse with a horned human figure and on the reverse with a human figure pulling an obscure object; a terracotta human head; a copper bull showing the dynamic mood of the animal and other copper objects including a pin; a terracotta feeding-cup with a cow's head on the rim; a terracotta graduated scale (incomplete) and an ivory comb (Plates XXIII-XXVIII). Both wheat and barley were found in the deposits of this Period, although the remains of the former were negligible. Barley was indeed the staple cereal. No evidence of rice was obtained.

Besides the above two principal parts of the metropolis there was also a third one—a modest structure situated upwards of 80 m. east of the lower city. The structure, of which the complete outline could not be recovered, was built of mud bricks of the usual Harappan size, 30 × 15 × 7.5 cm., and consisted of an impressive wall enclosing a room containing four to five "fire altars" of the kind referred to above. No other building, residential or otherwise, existed on this small mound. The absence of normal occupation debris suggests that the lonely structure with the altars was used for ritual purposes.

Another feature of the layout plan to which pointed attention may be drawn relates to the absence of habitational remains outside the walled area, except however in the restricted area to the south of the citadel, signifying that the city never outgrew its original layout. What does that imply in terms of economic and social pattern? On the other hand there are reasons in believe that during the later phases, when the fortifications, both of the citadel and the lower city, had been neglected, the settlement itself had shrunk.

The cemetery of the Harappan Period was located upwards of 300 m. to the west-southwest of the citadel on the present active flood plain of the river (Plates XXIX, XXX). Anciently however, it must have been beyond the reach of the annual floods. Three types of burials were attested: extended inhumation in rectangular or oval grave; pot-burial in a circular pit; and rectangular or oval grave-pit containing only pottery and other funerary objects. The latter two methods were unassociated with any skeletal remains.

Of the first variety, the recurrent features were as follows: on the floor of the oblong pit, the body was laid in an extended position with the head towards the north; around the head area, the funerary furnishings comprising pots and such objects of personal jewelry as bronze mirrors, etc., were deposited. The pit was thereafter filled with the selfsame earth. Three of the graves call for a special mention. In one of them the body was lying face down with its head towards the south, quite contrary to normal Harappan interment and showed a crouching position with folded legs. The other grave was lined with mud bricks (40 × 20 × 10 cm.) covered with approximately 2 cm.-thick plaster. The third grave showed evidence of two types of burials, one superimposed upon the other. The lower interment, consisting of pottery, was without any skeletal remains, while the upper contained the normal extended human body with pottery and one bead each of gold and carnelian around the neck. The grave in this case was quite large and had steps on the eastern side leading down to the grave floor. Useful information on palaeopathology was obtained from the skeletal material: evidence of trepanning was attested in a few cases.

Of the second variety, the grave pit was oval or circular and contained an urn and other pots including platters and dishes-on-stands clustered round the former. Besides pottery, some of the pits also contained such other objects as shell bangles and steatite beads.

Of the third variety, the grave pit was rectangular or oval in plan, with the longer axis oriented north-south as in the case of the first variety, but it was marked by the absence of skeletal remains. The grave goods consisted of pottery and occasionally of personal ornaments like beads and bangles. The striking feature about these pits was the filling which showed two stages: after the funerary deposit, the pit seems to have been left unfilled resulting in the accumulation of bands of fine sand and clay; at a later stage the remaining part of the pit was filled in by human agency with cloddy clay.

The occurrence of these three varieties of burial has posed problems of a sociological kind. Meanwhile, it may be affirmed that the grave goods obtained from each of these types are typically Indus. In their manner of occurrence the three types show a pattern, the significance of which cannot yet be appraised but is worth noting. Graves of the first and second variety occur in separate, reasonably defined, areas, the latter lying to the north of the former, while the graves of the third variety are found largely in the area of the first but occasionally also in that of the second.

What contributed to the end of the settlement still remains to be precisely determined. Meanwhile, it is sufficient to mention that one of the compelling reasons for the abandonment of the site was the drying of the river Ghaggar and the consequent denudation of the watershed by overgrazing and deforestation. The settlement must have been seriously affected by the shortage of a perennial water supply for both agricultural and drinking purposes. Environmental studies at Kalibangan (Raikes 1968a) have indicated a picture of alternating captures of the Yamuna by the Indus and Ganga river systems respectively. Among the series of alternating captures there was an eastward diversion to the Ganga system around 1750 B.C. which incidentally coincides with the abandonment of the site. We may not, therefore, look to foreign invasions or repeated high floods or transmutation to sub-Indus Cultures as contributory factors for the decline or fall of the settlement.

Coming to the dating of Period II, an inclusive bracket of 2300-1750 B.C. has already been postulated. As many as twenty-four samples (six from the Early, nine from the Middle and nine from the Late levels) have been dated by radiocarbon investigation (Agrawal and Kusumgar 1968b). Of these some of the samples from the Late levels, due to the absence of adequate soil cover, were not very reliable and have in fact given very young dates. The Late phase, therefore, shows a scatter. On the whole, the C-14 dates of Period II are internally consistent.

The story of the Harappan metropolis at Kalibangan is still incomplete. This interim report on our excavations and analysis is thus a tentative reconstruction of this important settlement.

The Harappan "Port" at Lothal : Another View

LAWRENCE S. LESHNIK

With a short note on Indian irrigation tanks by K.H. Junghans

To anyone concerned with the study of the ancient Orient, the site of Lothal on India's western seaboard has considerable importance.[1] Not only is it the southernmost outlier of the Harappan Civilization to have been thoroughly excavated, thus clearly demonstrating the vast extent of this civilization, but the claim that Lothal was a port settlement with direct trade relations with Mesopotamia has wide implications (Rao 1962, 1963b:179). Nearly half a century of research has unfortunately brought only rather scanty information on the nature of the Harappan economy and, particularly, its commerce. Until the excavation of Lothal, the direct evidence for Harappan maritime activity was limited to two depictions of boats (one on a pottery sherd, the other on a seal) from Mohenjo-daro, and likely as not these represent river craft rather than seaworthy vessels (Mackay 1938b: pls. LXIX:4, LXXXIX:A). Nonetheless, it has reasonably been thought that shipping did play a part in Harappan commerce, even if a doubt remains whether the Harappans themselves went to sea, or rather left the water transport of their merchandise to others (Piggott 1950:210; Wheeler 1960a:65).

The recent explorations by Dales (1962b) and the earlier researches of Stein (1931), which identified Harappan outposts along the Makran coast, lend support to the likelihood of sea-trade, and it is in this context of strong presumption but lacking evidence that the discovery of a large basin, interpreted as a dock, at Lothal assumes more than local interest. If the structure can indeed be considered a dock, then the arguments that the *Meluhha*, or perhaps *Dilmun* of Sumerian texts, was Western India gain much in weight (Oppenheim 1954, Kramer 1963b) [2] To some extent, the interpretation of Lothal as a port, and the basin structure as a dock, has already been accepted in archeological literature (Wheeler 1966:65); and indeed, the excavator of Lothal has claimed to have presented "conclusive proof" that ships were berthed there (Rao 1963a; Archaeological Survey of India 1960:66). Such certainty is rare to archeology, but this notwithstanding, doubts have been expressed[3] so that it seems apposite to view closely the nature of the Lothal site and, more particularly, the basin structure called a "dockyard."

Lothal (from the Gujerati *loth* = dead, hence meaning the same as Mohenjo-daro) is situated near Saragwala village, about fifty miles southwest of Ahmedabad (see Fig. A and E). It lies in a level plain between the Bhogava and Sabarmati Rivers and at present is some twelve miles distant from the Gulf of Cambay coast. The siltation rate of the Sabarmati delta is known to be rapid, so that in former times the site may actually have been nearer the sea. Lothal today is not linked with the Gulf by a waterway, but to the west of the site the map shows a depression which, we may suppose, formerly provided such a connection. The settlement area (according to the published plan about 600 by 850 feet) (Rao 1962: Fig. B1) and parts of the surroundings were subjected to large-scale excavation in eight campaigns between 1954 and 1962. In addition to several general articles, brief interim reports on each campaign have been published although the final excavation report has not yet appeared.[4] The excavator, S.R. Rao, has taken a particularly firm stand regarding the dock in these publications, and has thereby given the matter over to open discussion. I hope here to demonstrate that the basin at Lothal was

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not a dock at all, and to suggest some other possibilities for its functional interpretation.

The Harappan nature of the Lothal settlement is clearly established, with all the typical elements present, such as steatite seals bearing the characteristic script and designs, painted pottery, long chert blades, weights, some copper artifacts, architecture in burnt brick, and an excellent drainage system. There is evidence that copper, semi-precious stones, and possibly shell and ivory were worked at Lothal, but claims that the governing authority was secular and that fire worship was practiced require the further amplification of the full excavation report. The identification of an elevated area above the main settlement, which contains among other structures the remnants of a granary invites comparison with the cities of the Indus Valley, especially Harappa itself. The field crops on which the Lothal agriculture was based are thought to have included wheat, rice, and cotton.

According to Rao, it is a "certainty that the Harappans came to Lothal for trade or colonizing in 2450 B.C." (1962:15) and that when their settlement was destroyed, along with the Indus Valley cities in the wake of a great deluge, they moved to Rangpur and Bhagatrav. This would have been sometime after 2000 B.C. (Rao 1962:17; 1963a:204). The *Völkerwanderung* that brought the Harappans to Lothal is conceived of as a sea passage from the Indus. In the main, this argument is based on the supposed absence of Harappan sites in the tract between Sind and Lothal (i.e., in Kutch) and the establishment of a relative chronology that claims absolute priority in the region for the founding of Lothal (Rao 1957:82; 1965b:30). This dating is, however, itself questionable and exploration of the Kutch area has brought to light a number of Harappan sites there (Joshi 1966), so the arrival-by-sea theory will have to be reconsidered.

Walking through the excavated portion of Lothal one has the impression of being in the midst of a prosperous, not-quite-large village of the type still to be seen in Gujerat, excepting perhaps that the sanitary arrangements of four millennia ago were much superior to anything evidenced today. The extant size of the habitation appears rather too small to represent an "important metal working center and emporium for international trade" (Rao 1962:15), but it is thought that the original area of settlement was much greater (extending to a circumference of two miles), a large part having been later washed away by flooding. In trying to understand this development we are again handicapped by not having the full excavation report. Pending this however, the argument for a greater extension of the settlement is not quite convincing, for it depends upon the discovery of a brick-lined well some 500 feet south of the village (which might well have been in the fields) and the unsupported assertion of an extension to the north (Archaeological Survey of India 1960:17). Moreover, the habitation area appears to be well defined by antiflood walls (Rao 1962:Fig. B1), so that for the present purpose we take the limits of occupation as given above.

Abutting the eastern side of the mound is the structure that is of main concern here: a large, subrectangular basin set into the ground and revetted with walls of burnt brick. This is the structure called a dockyard, and upon it depends Lothal's identification as an international emporium, although it is interesting to note that Rao considered Lothal a port even before he discovered the basin (Archaeological Survey of India 1957:15). Yet, apart from the basin, there is very little about Lothal that would allow us to recognize it as a trading partner of Ur and Susa. The recovered objects of supposed foreign origin are very few indeed: they include one seal of the Persian Gulf type, a seal impression, some bun-shaped copper ingots, and sherds of a reserved-slip ware said to resemble one from Ur, Brak, and elsewhere (Rao 1965b:35). But this ceramic type has now been recognized at several Kutch sites, among them inland locations, so that it cannot be accepted as evidence for unique Lothal contacts with the West (Joshi 1966:64).

Lacking any analogies from South Asia itself, in what frame of reference is one to conceive the nature of an Indian port of the third millennium B.C.? Granting that some trade between the Harappan and Mesopotamian peoples is established, and that both civilizations were part of an ancient Persian Gulf *oikoumene*, the Mesopotamian example should prove instructive. In Sumer, docks were not merely places where merchandise was transferred to and from vessels, but the actual centers of commercial activity as well. Goods were exchanged and sold at the quay, for which purpose merchants had permanent establishments, and tavern-keepers were as familiar to the water-front scene then as they are in the ports of the world today. The harbor area was administered independently of the town proper and was separated physically from it. There the foreign traders also had their residences (Leemans 1960a:1; Oppenheim 1964:78, 116). At Lothal, by comparison, nothing of

this is evident; on the contrary, the basin is integrated into the settlement by its very proximity to the main residential area. That the Harappans otherwise divided their cities into sections is abundantly clear at Harappa, in the case of the workmen's quarters and also (as elsewhere) in the acropolis—lower town division. Unless, therefore, we suppose that foreigners were not encumbered with the same restrictions of residence as in Ur, their presence cannot be assumed, for there are no indications of such an enclave at Lothal. Neither are there any other remains, such as those of shops along the quay, that would bear comparison with the picture of the Sumerian harbor. At the least, it seems that the Harappan port had a different appearance from that of its Mesopotamian trading partner, but a likelier inference is that Lothal was not an emporium of the kind Rao envisages at all. (The granary, it is noted in passing, occurs as a fairly regular feature of Harappan settlements and is therefore not pertinent to the argument here.) In their turn, neither do the small finds encourage the view that Lothal was a center of international trade. The identifiable objects from overseas can be counted on one's fingers. Conceivably this might be because Lothal imports consisted largely of perishables. Sumer did export textile (Oppenheim 1954:6, 13), but whether to the cotton-growing Harappan peoples is questionable, and textual evidence for other perishable exports besides woolens and grains is lacking.

Rao suggests that besides the export of agricultural and marine (i.e., shell) products, ivory, and finished gemstones, Lothal imported rough stones and copper (1965b:30, 35). The export of cotton appears a reasonable supposition, since its presence at Ur has been identified, but it is less so to think that grains were sent to Sumer, whose field crops were its main natural source of wealth. The rough gemstones probably came from Central India, as Rao suggests, but whether the origin of the Lothal copper ingots is traceable to Susa remains to be seen (Rao 1965b:30, 35). Not far from Lothal in Rajasthan, we have evidence for the mining of copper ores at least by the middle of the second millennium B.C., if not earlier, and the two areas are in fact linked by a common ceramic that might imply close cultural association.[5] Moreover, there is reason to suppose (if the identification of Meluhha with India is accepted) that copper was an Indian *export*, not *import*, in Sargonid times. Also from Meluhha, Sumer received carnelian beads (Oppenheim 1954:14; Leemans 1960a:10, 33) (sometimes in the characteristically Harappan kidney shape), which could have come from Lothal, but might as well have originated in the Indus Valley; Chanhu-daro, for example, is also known to have been a bead-working center. One material found in Mesopotamia which is likely to have come from the region of Lothal is the chank shell (Hornell 1951:239), but this does not yet make Lothal a port. Seven terracotta models of boats recovered by the excavator are, unfortunately, too indistinguished to bear on the question.

On the whole, there is not much evidence for an extensive or regular overseas trade conducted by the Harappans, and it would be curious if the little there was lay in the hands of relatively minor settlements such as Lothal. In an age of cities, it is difficult to imagine a village as an international trade center. Admittedly, arguments *ex silentio*, as are some of the above, do not bring one far, but apart from the basin, there is little else upon which the claim to Lothal's being an international port rests. To this basin I now turn.

Evidence for actual docks in the ancient world, against which the Lothal basin can be measured, is sharply opposed. On the one hand, in Mesopotamia, none at all is known, a fact which may mean either that none has yet been discovered, or that none existed, i.e. that mooring arrangements were of the simplest kind. The absence of any textual reference to them points to the latter alternative (Oppenheim 1967: personal communication). On the other hand, the complex harbor at Pharos (modern Alexandria) which dates back to *c.* 2000 B.C. was so skillfully constructed with breakwaters and jetties that an expert, Sir L.H. Savile, has called it the work of a genius, equal to the standard of a modern engineer (1941:210, 214). By contrast, the Lothal basin is a quite elementary structure, and if meant to function as a dock, then it was very poorly designed. Yet the excavated remains of Harappa and Mohenjo-daro leave us in no doubt that Harappan competence in civil engineering was the equal of any in the third millennium B.C. Rao himself has pointed out that the Bronze Age knew no other port similar to the one at Lothal (1965b:32), and though Lothal could be a unique instance, this observation requires a cautious approach to the interpretation of the structure.

As mentioned, it is a recessed basin, subrectangular with its long arms running east-west and measuring 710 feet. The southern end is 116 feet long, that at the north, 124 feet. It is revetted on all four sides with a

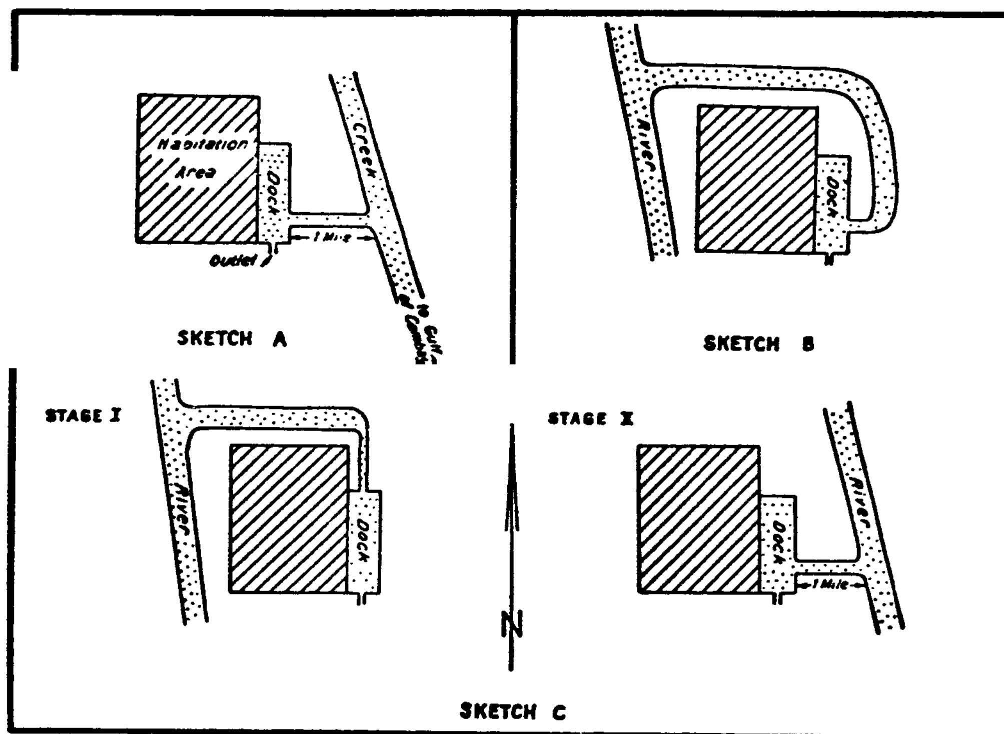


FIGURE 26.1. *Sketch maps of Rao's various reconstructions for the "Dockyard" at Lothal.*

continuous dry masonry burnt-brick wall, four courses wide, which at its greatest extant depth reaches to fourteen feet (Archaeological Survey of India 1959:13). The walls are vertical on all sides and there is no access to the basin in the form of steps. Towards the southern end of the eastern embankment there is a broad and relatively shallow gap. This, it has been supposed, was the inlet channel of the dock. Leading off from the southern wall is a narrow, brick water-passage, said to have functioned as a spill channel when fitted with a sluice-gate (Archaeological Survey of India 1959: pl. XIVB).

Originally, it had been suggested that ships were sluiced at high tide from the Gulf of Cambay to the dock through the inlet channel (probably via a creek to the east of the basin—see Fig. 26.1, sketch A) (Rao 1962:17). This explanation raised some difficulties, since it was not understandable why the inlet should be in such a position that entering ships would have to take a 90° turn in order to berth.[6] Moreover, the topographic map indicates a depression (possibly a former river bed) to the *west* of the mound, that is, with the habitation area placed between the dock and the water source. Futher study of the problem convinced Rao that ships entered the dock from the river to the *west* (not as first thought, to the *east*) by way of a navigation canal skirting the northern margin of the settlement (Fig. 26.1, sketch B) (Archaeological Survey of India 1963:7). This explanation satisfactorily accounted for the relative positions of river, habitation area, and dock, but suffered the disadvantage of compounding the awkwardness of the approach to the dock. In the most recent and complete discussion of the Lothal port, Rao (1965b) has also sought to resolve this difficulty. We are told that the dock was used in two stages. During Stage I ships entered from a canal running east-west along the northern edge of the settlement (as in Fig. 26.1, sketch C) and opening into the *northern* embankment of the dock. This wall originally had a fifty-five foot wide gap in it (not previously reported), which would represent the inlet of Stage I. Eventually, an "unprecedented flood" blocked the mouth of the original flow channel, and brought about a shift in the river to the east of the settlement. Thus in Stage II, a new inlet channel was built into the eastern embankment to accord with the changed position of the water course (Fig. 26.1, sketch C). Rao explains that in Stage II, the inlet channel was too shallow and narrow to accommodate large ships, but initially (Stage I) the structure was designed "to

sluice ships 18-20 meters long and 4-6 meters wide. At least two ships capable of plying the high seas would pass simultaneously and manoeuvre easily. In Stage II however, only flat bottoms could enter. At this period, the seafaring vessels would be berthed in deeper waters and their cargos trans-shipped by country craft" (1965b:35). This most recent interpretation is hard to reconcile with previous ones, for prior to the report of the northern inlet, Rao wrote that the entrance of sea-going vessels through the eastern inlet, had been "established beyond doubt" (Archaeological Survey of India 1962:10). He does not tell us why he changed his mind, but one can guess the nature of the eastern gap had something to do with it. With hardly three feet separating the sill of the gap from the rim of the adjacent walls (Archaeological Survey of India 1959: pl. XVB), there is patently inadequate clearance for even a small fishing vessel of the type that today plies the coast, but is not seaworthy. Such boats, weighing about 100 tons, have a draft of some four feet. (It has been assumed by Rao that the basin's rim was at one time higher, yet he does not cite the relevant evidence (1965b:32, 35).

Now the point in having a completely enclosed dock with its water-level controlled by a spill channel is to enable the flotation of ships when the tide is out. If this is not required, then a basin for arresting water becomes unnecessary, a simple open wharf sufficing for mooring arrangements. Hence in Stage II, the dockyard would be serving no purpose, if the sea-going vessels were left to fend in the pull of the tides. Rao has drawn attention to what he supposes to be a parallel to the Lothal dock in the present-day nearby dock at Ghogha (1962:18). The significant point about this other dock, it seems to me, is precisely that it is not an enclosed basin, but merely a quay alongside which ships are berthed. Evidently, the flotation of ships at ebb tide is not considered essential here, and as Rao mentions, they get stuck in the mud, which is apparently no particular disadvantage.

At this point, it would be well to look somewhat more closely at this basin. I am not able to comment on the reported inlet at the northern embankment since my efforts to locate it on the site itself were unsuccessful, and Rao unfortunately has not yet published any illustration of it. The eastern inlet however is clearly depicted (Archaeological Survey of India 1959: pl. XVB). To be added to what has already been said about this is the observation that the jambs in either side of the gap show no evidence of being planned. They terminate in irregular, uneven steps, so that the opening looks to be much less the result of intent than a chance breach effected by brick robbers. The level of the sill is too high to be consistent with a navigable inlet, and there is no trace of any provision for affixing a sluice gate, especially one spanning twenty-three feet and sturdy enough to withstand an enormous internal water pressure at ebb tide.[7] During the last-but-one excavation campaign, confirmation for the presence of an inlet channel at this point was sought by cutting across its suspected alignment some yards distant from the basin. The profile revealed a concavity now filled in with silt, and a photograph of this has been published (Archaeological Survey of India 1962:10, pl. XVIIA). In the photo, the depression appears quite shallow, probably too much so, again, to have permitted the passage of ships. Successive deposits of silt and fine sand as well as the presence of sea shells do not necessarily point to the flow of tidal waters, as maintained by Rao. At one time, the Nal Lake was still connected with the sea (Government of India 1879:16), this entire area would have been subjected to flooding, and deposits of the kind observed readily left in surface depressions.[8]

Leading off from the southern embankment is the presumed spill channel. The conduit, constructed of burnt brick, has been traced in its length for several yards. The water passage is about one yard wide and contains at the lower end, near the basin, a stepped descent that serves to decrease the water velocity. The orientation of the steps and the gradient of the conduit as a whole is *toward* the basin. Obviously, then, its purpose must have been just the reverse of an outlet and the staged passage affords another illustration of how thoroughly competent the Harappans were in the field of civil engineering. With the conduit serving as an inlet, the basin cannot have been a dock.

During the excavations seven "anchor" stones were found, five of them coming from the basin area (Rao 1965b:35). Six of the seven stones are discoid with central holes and another is pyramidal, with holes in the apex. The largest of the stones has a diameter of about sixteen inches, making it only doubtfully heavy enough to have served as an anchor by itself, while a series of them would have been unwieldy. Similar stones have been found in and around the Nal Lake, and have also passed into the local folklore as anchor stones though this does not, of course, assure their identification (Government of India 1879:16). Ring-stones of this kind are further known from Harappa

and Mohenjo-daro where they were thought to have been either architectural units, used in erecting pillars, or, in another view, cult objects recalling Yonis (Marshall 1931:I, 61-63; II, 473; III, pl. CXXX, 7, 9). The collection of seventeen ringstones in a single place at Mohenjo-daro might point to some such function, but then only secondarily. At Lothal, we have a hint of their primary use by the occurrence of five of them in or near the basin. I would suggest that they served as counterweights for the arm of a *shaduf* (in India, called *lat*, or *piccotah*). The shaduf is a water-lifting device of considerable antiquity, consisting of two poles, one of which is stationary and erect, while the other, attached to it, moves in a vertical plane. A bucket is suspended from the latter at one end and counter-balancing weights are affixed to the other (see Fig. 26.2, after Buckley 1893:3). It is operated by a single man who pulls the empty bucket down into the water of a well, tank, or river, and then retrieves the full bucket with ease by letting the counterweight do the work of lifting. The shaduf is prominently featured in Egyptian rural scenes and was also used in Mesopotamia. In Mohenjo-daro there is a linear representation of a man using the shaduf, so that its presence is documented for the Harappan civilization as well (Marshall 1931:389). Marshall describes the Mohenjo-daro ringstones as having slots that were used to fasten the stones to something that passed through the central aperture. This could well have been the arm of a shaduf, to which the stone weights were lashed by rope or leather thongs. The shaduf is still employed near Lothal, although the stones are no longer pierced, but simply secured with rope. Pierced stones continue however to be used in this way in Eastern India.[9] At points along the embankment of the basin, Rao has identified post holes, which he has associated with mooring posts, but their use for the vertical arm of the shaduf would also be consistent with the opinion expressed here. Some further support may be lent to this by the observation that the shaduf does not usually lift water more than fifteen feet, and that the depth of the basin is in close accord with this simple physical limitation.

We have here then, a long, narrow, and relatively shallow enclosure, revetted in brick, with a single inlet channel and perhaps a line of shadufs standing along the embankments. To what use was it put? A single answer is likely to be only part of the story since the evident possibilities are not mutually exclusive. The character of the Lothal settlement was critically reviewed at mid-stage in its excavation by U.P. Shah,

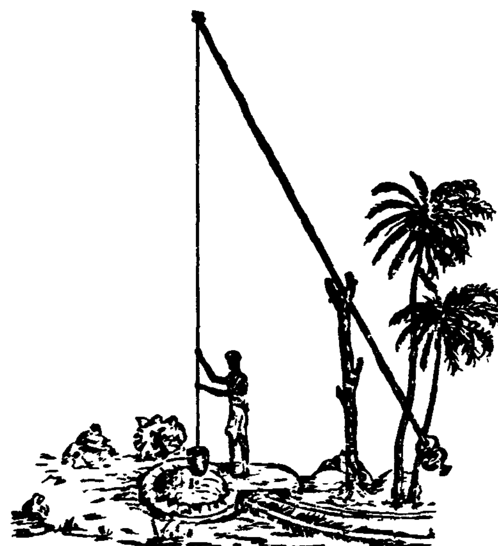


FIGURE 26.2. *A sketch of a* shaduf *(after Buckley 1893:3).*

who expressed the opinion that the basin served as a reservoir for drinking water. Professor Shah particularly emphasized the present-day scarcity of potable water in the region (1960:312, fn. 9), and this is evident when one sees the saline efflorescences that blanket the wastelands in the area. (Only two wells have been found at Lothal, one of these being beyond the habitation area. The supply of water from wells appears to have been quite limited.) As professor Shah conceived it, the reservoir would have been solely rain fed, though now the identification of an inlet channel shows that water was also drawn from the river. Today, nearly every town and village in the region has its own reservoir, which is used soon after the rains, mainly for bathing and washing. But as wells and streams turn salty with the advance of the hot weather the reservoirs become the sole source of drinking water. Most of them, it is true, are mere ponds, although some do have brick wallings. These again usually have steps leading down to the water, and their absence at Lothal has been used as a decisive argument in rejecting the implied parallel (Rao 1963a:179). Steps are not, however, an invariable feature of the tanks; in 1879, the town of Dhandhuka, not far from Lothal, had a masonry tank that enclosed some 20 acres but was without steps (Government of India 1879:19). The absence of an entry to the Lothal tank might hinder its use for many purposes, but not, with the help of the shaduf, for the storage of drinking water. From all else that we know about the Harappans, it would not be

surprising to learn that their notions of water pollution were clearer than those prevalent today.

The basin could have been, and probably was, used as a source of drinking water, but I believe its primary purpose was to store water for irrigation. If future research can substantiate this identification, in preference to the port theory, then the contribution of the Lothal excavations to our knowledge of ancient India is in no way diminished, for in losing a dock, we gain the first real insight into the Harappan agricultural system. To date, very little is known about the agrarian basis of this civilization, and the relationship the larger urban centers maintained with the rural hinterland.[10] It is generally supposed that the people of the Indus Valley settlement practiced some kind of irrigation, although direct evidence has been lacking (Wheeler 1960a:67; Drower 1958:522).[11] Lothal has perhaps now provided some.

Before the days of the great canalization projects in the Punjab and South India, indigenous irrigation was largely based on tanks, and still today large parts of the South are dependent upon it. Some writers have supposed that this method orginated in India. "Tank" in the Indian usage refers to a reservoir that stores water either by damming a stream at a conveniently broad, high place, or by the diversion of its waters to some depression (Wilson 1903:153), either natural or artificial. (It is this latter type that is exemplified at Lothal.) The walls of these tanks are frequently only earthen but also sometimes of masonry. Some tanks are great in size, covering 9-10 square miles, while the storage capacity of others suffices only for limited local use (Buckley 1893:6).

As far back as Rigvedic times (in the latter half of the second millennium B.C.) tank irrigation was known, although well irrigation was perhaps more common. In the later days of ancient India, the construction of tanks came to be considered a meritorious deed and belonged to the list of royal duties. In the Mahabharata (II.5.77) a king is asked: "are large and swelling lakes excavated all over thy kingdom at proper intervals, without agriculture being in thy realm entirely dependent on the showers of heaven?" (Bose 1961:135). The earliest specific mention of an irrigation tank comes from Junagarh (in Kathiawad) and dates to about 300 B.C. (Venkayya 1906:202, A.L. Basham 1967: personal communication). But recently, it has been suggested that tank irrigation came to South India together with the megalithic mode of burial, and this would add perhaps a further 100 years to the age of tanks in India, though still making them 1500 years later than the Lothal example. The evidence for the temporal association of megaliths and tanks is however entirely circumstantial, resting on the coincidence that tanks are often found in the vicinity of the tombs (Banerjee 1956:23). Since there are, at one counting, over 40,000 tanks in South India (Burn 1908: III, 323), the association needs to be more firmly established before it can be accepted.

The region of Lothal is fertile, producing with consistency cotton, wheat, barley, and other crops, even under wasteful methods of cultivation (Government of India 1879:4). Annual rainfall amounts to some thirty inches, which in *quantity* would suffice to meet the crop refinements. However the erratic *distribution* of the rains makes agriculture precarious without artificial means for regulating the distribution of moisture to the crops. In the last century, tank irrigation was resorted to mostly during the cold months of November and December, in order to bring the rice crops to maturity. A cultivator commonly hollowed a part of his field for a pond, and allowed this to fill with rain water. Then, if the later rains failed, he would carry the pond water to the rice beds by means of a channel or water lift (Government of India 1879:51).

In this light, the fact that Lothal is one of the two places in India (the other is Navda Toli) for which the early use of rice is attested assumes renewed significance. The presence of the granary at Lothal (and much else that we know of the Harappans) implies that agriculture was subject to some sort of centralized control that must have extended to the irrigation system. Given then (1) that rice was cultivated and that this required the use of stored water and (2) that agriculture was governmentally regulated, it would be an administratively sensible approach to irrigation to construct just such a tank as we have at Lothal. Throughout the ages, irrigation in the Orient has been a matter of governmental concern, and it is not likely to have been otherwise at Lothal.

In lieu of factual evidence, it might be supposed that the inlet channel was connected with the tank at one end and the river, which once flowed west of the mound, at the other. A weir thrown across the river would divert water to the inlet, the flow being controlled by staging and, finally, by a sluice gate. The fittings for such a gate have been identified at the basin end of the channel. One of the great difficulties with such tanks is that they retain all the silt of the stored

water, and must either be periodically cleared, or abandoned. The use of the bricks at Lothal eased the cleaning task, provided for a long period of usage, and protected the basin from the erosive force of the water entering through the inlet. When required, the stored water was lifted out of the tank by shadufs and brought to the fields by an interconnecting canal system. Already well acquainted with drainage canals in their settlements, the irrigation canals would have presented no special problem to the Harappans. The one well that was found beyond the settlement area might have supplemented the tank irrigation, a practice common in India (Burn 1908: VIII, 318).

In summary, the identification of Lothal as a port of international commerce seems questionable. The arguments favoring it are based on slender evidence, and ultimately, they rely upon the demonstration that the basin-like structure abutting the habitation area was a dock capable of receiving sea-going vessels. In my opinion, this has not been adequately shown, for the presumed opening for ships in the eastern embankment appears to be due more to the depredations of brick robbers than intentional design, and the conduit identified as a spill channel is more likely to have served as an inlet for water from the river. The alternative explanation I offer for the basin is that it served as an irrigation tank and, secondarily, as a source for drinking water.

Admittedly, this view also lacks the weight of decisive evidence, but it seems at least to be reasonably in keeping with the general rural character of the Lothal settlement as we know it. Rao's significant work has opened up several new lines of inquiry, the most important of which may lead to valuable insights into the Harappan agricultural system.

A NOTE ON THE LOTHAL TANK AS AN IRRIGATION RESERVOIR

K.H. JUNGHANS

The question has been raised whether the basin at Lothal might not have served as an irrigation reservoir. It is true that the fields surrounding Lothal could have easily been irrigated by canals. Why then the necessity of leading water through a conduit into the basin, then raising it and again bringing it onto the fields along a canal system? The answer to this is coupled to another query: were technical facilities sufficiently advanced to enable large amounts of water to be lifted out of the basin in a short period, in order to fill the irrigation canals?

In this area of scanty rainfall, rivers and canals often run dry during the summer months. The example mentioned above for Gujerat, where tanks are built prior to the monsoon rains for the storage of water to be used later, can be extended to many parts of India. In Orissa, the construction of such tanks is in fact part of an official famine prevention policy. It is, however, remarkable that the Lothal tank was brick-built and not simply made of earthen walls as is generally the case today in India. Besides facilitating cleaning and preventing erosion, as already suggested, the brick walls also reflect something of the irrigation techniques employed. If made of earth, the walls of a basin would have to be much sloped to prevent soil movement. Now if the fields to be irrigated lie lower than the tank, the outflow of water can easily be regulated by sluice gates. The sloped sides would be no hindrance in this case. However, if the fields are at the same height as the basin, then the water has to be mechanically raised. Today this is frequently done by pumps or Persian wheels, but formerly, the shaduf system prevailed. Difficulties arise however if shadufs are to be used over sloped walls. Hence today, where in use, the shadufs are primarily erected at wells that are masonry constructions. The brick walls of the Lothal basin would have been admirably suited for the use of shadufs at the water's edge, and we would adduce this fact in support of the opinion that this system was actually used at Lothal, as evidenced by the ringstones. We now turn to consider certain economic aspects of the problem.

It would be difficult to state with certainty just which crops would have been irrigated from the tank, and we can offer here only some suggestions. What is clear, however, is that the size of the tank is sufficiently large to permit the use of a number of shadufs, operated perhaps by a social group that specialized in this work. Still today one finds, for example among the Mundas of eastern India, the shared use of irrigation works. Four to six families of a village dig a well on communal property and receive in return a plot near this water source. Shadufs are erected and each family draws water for its fields from the well. In a recent study of such a system near the Rourkela steel plant in Orissa, it was determined that an irrigated area of 0.3 to 0.6 acres of vegetable land sufficed to employ a family fully.

The Lothal tank had a capacity of about 5,000 cubic meters, and the flow of the water into it was assured for

about five to six months. This would have permitted refilling three to four times yearly, so that a total of 15,000 to 20,000 cubic meters of water was available for irrigation purposes. For vegetables, we assume an average growth period of some eighty days during which period the plants require 500 to 700 mm rainfall. Now, generally irrigated fields are capable of producing three vegetable harvests a year, thus requiring 1,500 to 1,800 mm of water. The actual rainfall in the Lothal region today is 700 to 800 mm, and we assume a similar precipitation in former times. The tank capacity would have sufficed to provide the deficit moisture for vegetable gardening. As we understand it, the tank had a dual function: (1) it provided the farmer with a kind of insurance in the event of a monsoon failure and (2) permitted vegetable gardening throughout the dry season. If the tank was filled four times yearly, providing 20,000 cubic meters of water, this together with the normal rainfall would have made eight to twelve acres available for vegetable gardening.

This area would give work to twenty or twenty-five families. Relatively small as the area is, the resulting vegetable harvest would represent an important culinary addition, not only balancing but also relieving the monotony of a grain diet.

Whether the irrigation system was also used to water grain fields is another matter. Grain cropping in earlier times required extensive areas, in relation to which the capacity of the Lothal tank seems small. But even if field crops were not solely dependent upon irrigation, it is certainly possible that the stored water was used as a hedge against dry periods later in the year, as already suggested above.

NOTES

[1]A concise version of this article was read at the 27th International Orientalist Congress at Ann Arbor in August 1967.

[2]For a summary of the arguments favoring the identification of Western India with Meluḫḫa see Leemans (1960a). See also two recent statements: Buchanan (1967) and Mallowan (1965).

[3]Several Indian and European scholars have privately expressed to me doubts about the dock interpretation. To my knowledge, the sole other publication questioning it is by Professor U.P. Shah (1960).

[4]Reports on the Lothal excavation will be found in *Indian Archaeology. A Review* published by the Archaeological Survey of India in the following issues. 1954-55:12, 1955-56:6; 1956-57:15; 1957-58:12; 1958-59:13; 1959-60:16; 1961-62:10; 1962-63:7 See also S.R. Rao 1957, 1961a, 1962, 1963a, 1963b, 1965b, 1973.

[5]The ceramic is the black and red ware found at many sites in Central India, but principally associated with Ahar, in Rajasthan, where copper was anciently worked. See *Indian Archaeology A Review*: 1961-62:45.

[6]This point was made by Mr. P. Oza, at the time Director of Ports in Ahmedabad, when he prepared his report on his observation at Lothal at the excavator's request. The present Director of Ports, Mr. Mehta, has kindly made Mr. Oza's report available to me, and in Baroda I had the opportunity of discussing it with him Mr. Oza did not commit himself, either to the report, or during our talk, to the view that the structure was a dock. In quoting him as being "definitely of (this) opinion," Rao is mistaken (1962:18). I wish here also to acknowledge the readiness with which Mr. Buddhbhatt, Executive Engineer in Ahmedabad, discussed the problem with me.

[7]The small block of bricks abutting the exterior of the eastern wall near the gap is not with certainty associated with it.

[8]There is also some doubt whether the shells recovered were actually those of salt and not fresh water species.

[9]This information comes from Dr K.H. Junghans of Heidelberg

[10]Some very interesting thoughts on such questions have recently been recorded by Fairservis (1967).

[11]For a somewhat different interpretation see Lambrick (1964:75)

Lothal: A Gateway Settlement of the Harappan Civilization

GREGORY L. POSSEHL

Lothal is one of the most fascinating settlements of the Indus Civilization. The material evidence generated by large-scale excavations there, the internal structure of the settlement, its setting within the regional geography of Gujarat and the Indus Civilization as a whole provide important data for understanding South Asia's first civilization.

Lothal was discovered by S.R. Rao (1957, 1962, 1973) of the Archaeological Survey of India in November 1954 and excavated for seven consecutive seasons. Stratigraphically the site has been divided into five building levels each separated from the others by what the excavator refers to as flood deposits. In the first four levels, numbered I-IV from bottom to top and designated Lothal A, were found ceramics and small finds of Mature or Urban Harappan character, while the fifth and final level (Lothal B) can be termed Late or Post-urban Harappan. Comparative typology and radiocarbon dates suggest that Lothal A can be dated between 2100 and 1850 B.C. (2550 to 2160 b.c.)[1] and that Lothal B ends one-hundred to one-hundred and fifty years later.

The first building level, with its limited architecture and no signs of settlement planning, suggests that Lothal was at that time a small "pioneer" settlement. Then in the succeeding three levels the settlement is expanded in size, architectural differentiation can be noted, and there are definite indications of community planning. In the fifth level Lothal is once again reduced to a small settlement and the architecture suggests a "squatters" occupation just before the final abandonment of the site. The following discussion will be limited to the "mature" phase levels II, III and IV.

It has been asserted that Lothal was a city (Rao 1973:52, 61). It is, however, a surprisingly small settlement, although there can be little doubt as to its internal complexity, or differentiation. Judged from published maps and my own visits, the entire site is on the order of 300 by 250 meters, approximately one-fourth the size of Mohenjodaro or Harappa (Rao 1973:63). A population of between one and two thousand would therefore appear to be a reasonable estimate for Lothal. A settlement of this size is probably best referred to as a "town," reserving "city" for the vastly larger places. This distinction is also corroborated by Lothal's internal structure.

Lothal has been divided into three architectural areas or "districts": the Citadel, the Lower Town and the Dockyard (Fig. E).

The Citadel is an imposing area artificially raised above the remainder of the settlement on a mud brick platform. This area occupies the southeast quadrant of the site and is 48.5 by 42.5 meters in size. On the Citadel are streets at right angles to one another which separate blocks of mud and baked brick structures, the functions of which are incompletely known. One of these blocks, with very thick mud brick foundations, has been interpreted as a warehouse. Another large structure, not far from the warehouse, has been suggested as having been a merchant's house. Of further note is a row of twelve bathrooms with elaborate drains. The close parallel to the bathrooms surrounding the Great Bath on the Stupa Mound at Mohenjodaro is well known.

The Lower Town is also divided into blocks separated by streets at right angles oriented to the cardinal directions. Only six of these blocks were excavated. The Lower Town appears to have been the location of

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domestic quarters at Lothal as well as the district for several manufacturing processes. These will be discussed in greater detail below.

The so-called "Dockyard" is probably the most controversial feature at Lothal. This structure is located on the eastern side of the site. Constructed of baked brick, the "Dockyard" is nearly rectangular in shape and 219 by 37 meters in size. The brick walls of the sunken enclosure are 4.5 meters high. A platform borders the town side and permits easy access to the warehouse area on the Citadel.

The precise function of this brick lined enclosure is still open to question. No conclusive demonstration of its use as a dockyard has been made, and there is an important critique which suggests that it is a tank for the storage of irrigation water (Leshnik 1968b). In fact, neither of these alternatives, the tank versus the dockyard, is fully convincing. There is serious question as to the necessity of a tank for irrigation water storage in the Lothal area. Seasonal inundation is sufficient for one crop and ground water is extremely close to the surface at other times of the year. The means of lifting water from the tank has yet to be explained. Leshnik suggests a *shaduf* system (Fig. 26.2), positing that the "anchor" stones found in the enclosure were counterweights. The author has seen these stones at Lothal where they are displayed on the site. The holes through them are quite small: on the order of two or three inches in diameter. This is a size appropriate for a rope, but not for suspension as a counterweight on a branch two or three inches in diameter. The fact that the tank borders one side of the settlement also limits its potential for irrigation. It would have been much more efficiently placed had it been constructed some distance from the settlement where water could have been drawn from it for fields all around. There are no stairs or other means of access to the interior of the enclosure. This means that it could not have been used for bathing and laundry except with difficulty. Finally, there is a well on the loading platform between this structure and the Citadel. Water for use in the settlement would have been taken from this source and not from the open, stagnant and possibly fouled tank. Considerations such as these can be used to at least cast doubt on Leshnik's hypothesis just as his arguments cast doubt on the contention that the sunken brick lined enclosure was a dockyard.

There is a fundamental question as to whether a large dockyard would have been necessary, even if Lothal had been a port. Today ships come into riverine ports on and adjacent to the Gulf of Cambay and anchor at high tide on mud flats where they are loaded and unloaded without other facilities. In addition the placement of the enclosure, if it was used for docking ships, is odd. It is on the side of the settlement opposite the primary river access. Thus ships would have had to come past the settlement and to have made at least one right angle turn in a narrow channel. In fact at one point in Rao's (1965b) reconstruction two right angle turns in such a channel are called for. This was certainly not a very efficient system. How much more logical it would have been if the dockyard had been built on the river side of the town where an uncomplicated entrance system would have sufficed. Finally, access over the peripheral enclosing wall to the interior of the facility is extremely shallow. Excavation has shown it to be on the order of seven brick courses. This would admit water craft with less than a meter draft, probably excluding sailing craft appropriate for ocean-going maritime trade.

The interpretation of the Lothal "Dockyard" is far from resolved. But whether it is or is not a "Dockyard" or a tank makes little difference in terms of this settlement's place in the cultural geography of prehistoric Gujarat. The differentiated nature of the architecture, and the presence of identifiable districts suggests a complexity for Lothal. This is revealed in the type and variety of the artifactual assemblage as well. Present at the site was a wide variety of raw materials, objects in various stages of manufacture, finished products as well as waste and residual materials. Stone anvils, bronze drills, crucibles, slag, remnants of conch shell, copper ingots, whole tusks from elephants (and others partly sawn) are but a partial list of the items relating to specialized crafts. Of particular note in this regard is the presence of a bead factory in the Lower Town. In this structure several rooms of small size surround a central courtyard. Within the latter there is a raised working platform with an anvil. The archaeological deposits associated with the factory contained many unfinished beads, drills, and other tools as well as bead materials and hundreds of finished ornaments. A special furnace for heating carnelian to enhance its color is also a part of the complex. Beads of many forms made from carnelian, crystal, jasper, opal, steatite, paste and the like were manufactured on a very large scale at Lothal, suggesting that this manufacturing process must have been one of the most important activities at the site.

Lothal was a carefully planned settlement, ordered

into districts in a way which led to an economy of space. There were areas, such as the bead factory, a smelter, a place within which shell bangles were made, that suggest craft specialization and further reinforce our sense of an overall order to the layout of the town. The haphazardness associated with settlements which grow by accretion is, if judged by the excavated areas, not a part of this plan.

It can be reasonably inferred that his diverse and evidently voluminous, specialized craft activity produced much more in the way of finished products than the settlement itself could have consumed. Lothal was, after all, a place with a relatively small population. We must therefore look for the "market" to which at least a portion of the finished products must have been directed, as well as the areas from which the raw materials documented on the site were derived. We know that Lothal was not situated in a location particularly suited to resource extraction. Its immediate environs in fact produce almost none of the raw materials (copper and other ores, stones, shell, ivory, etc.) which played such a key role in the life of this settlement. The nearest copper comes from Rajasthan, as does steatite. Agate is found on the Narmada and in Saurashtra and Kutch. Shell is found along the coast of the Arabian Sea. Ivory was undoubtedly available locally but its acquisition would have involved parties of hunters who had to go out in search of elephants. Lothal should therefore be seen as sitting at the hub of what must have been a complex and continuous exchange network. The nature of this exchange is not fully understood; however, it appears to have involved not only the inhabitants of this settlement but the other non-Harappan folk around them.

It is known that the large north Gujarat plain, to the north and east of Lothal, was inhabited by seasonal hunting and gathering peoples at the same time as Lothal was occupied. This has been demonstrated by excavations at Langhnaj, a "microlithic" site fifteen miles north of Ahmedabad at which one radiocarbon date and some significant pieces of material culture attest to a date within the third millennium (Clutton-Brock 1965; Ehrhardt and Kennedy, 1965; Sankalia 1965). The C-14 date—TF-744, 2040 ± 110 B.C. or 2600 to 2180 b.c. (Agrawal and Kusumgar 1969)—is from a bone sample and is thus somewhat less reliable than most radiocarbon determinations. However, neither the stratigraphy nor the material remains at the site contradict it.

Of considerable importance to the present discussion is the occurrence of what appear to be trade items at Langhnaj. There is a 98.12 per cent pure copper knife, black and red ware pottery which is typologically akin to a ware found at Lothal, and disk beads of Harappan type. These items all suggest contact between the inhabitants of Langhnaj and the Harappan Civilization.

Langhnaj is probably not a unique site in north Gujarat since there are several hundred such hunting and gathering settlements there. It should be noted as well that the north Gujarat plain was never settled by "chalcolithic" settled village agriculturalists and exploration in this region has certainly been intensive enough to have recorded such sites had they been present. In discussing his own work Leshnik notes:

> The thought which initially gave rise to this exploration was that there was a reasonable chance of finding mounds of the Chalcolithic Period in northern Gujarat and the Luni River region of Marwar. Yet the absence of even a single prehistoric mound is the first conclusion to be noted. We are convinced that our survey procedure would have located such mounds, had they been present (Leshnik 1968a:308-9).

He goes on to hypothesize that the early agriculturalists never penetrated the northern plain because of the absence of the moisture-retentive black cotton soils to which their dry farming techniques were adapted. This seems to be a most reasonable view with which I can take no issue. In addition, however, I would suggest that the relationship which had evidently formed between the Harappans and the hunting and gathering peoples, as documented by the material ties between Lothal and Langhnaj, made it advantageous for the Harappans to leave these folk as undisturbed as possible. What is suggested here is that there had evolved a complex exchange system between a food producing group with a sophisticated technology and a demand for raw materials, and a hunting and gathering group with a need for finished goods plus access to raw materials as a medium for exchange, such that the agriculturalists found it advantageous not to "colonize" North Gujarat, and thereby risk upsetting the balance of this exchange relationship. There is a modern ethnographic analogy within India which, with qualification, yields additional insights into the possible nature of this arrangement in antiquity.

> Rather than being independent, primitive fossils, Indian hunters-and-gatherers represent occupationally specialized productive units similar to caste groups such as Carpenters, Shepherds, or Leather-workers. Their economic regimen is geared to trade and exchange with the more complex agricultural and caste communities within whose orbit they live. Hunting and gathering in the Indian context is not an economic response to a total, undifferentiated environment. Rather, it is a highly specialized and selective orientation to the natural situation: where forest goods are collected and valued primarily for external barter or trade, and where necessary subsistence or ceremonial items—such as iron tools, rice, arrowheads, etc.—are only obtainable in this way. Far from depending wholly on the forest for their own direct subsistence, the Indian hunters-and-gatherers are highly specialized exploiters of a marginal terrain from which they supply the larger society with desirable, but otherwise unobtainable, forest items such as honey, wax, rope and twine, baskets, and monkey and deer meat. Unlike the Australian aborigines or the Paiutes, their economic process and well-being are dependent on the barter of these items for the crops and crafts of their more complexly organized plainsmen neighbours. The economic activity of Indian hunting-and-gathering groups is more akin to the specialization of caste hereditary occupation, than it is to the generalized environmental response of the Australian or Paiute (Fox 1969:141-42).

I would suggest that the relationship outlined by Fox has probable parallels in the prehistoric context under discussion. In that case the character and location of Lothal and the presence of hunting and gathering groups shown to have been in contact with metal and pottery-using peoples serves as evidence that the relationship suggested by Fox has its roots in a prehistoric context. Of course, the Fox statement must be modified in some ways. For example it should not be implied that the hunter-gatherers under discussion in antiquity " ... represent occupationally specialized productive units similar to caste groups. ... " A measure of specialization can be accepted for them but any direct comparison to caste must be moved aside for the moment. Similar references to caste in the agricultural communities should be ignored. The general nature of Fox's model, which includes many blanket statements covering a variety of specific situations, should be tailored to the specific conditions under examination. For example, virtually all hunters and gatherers in prehistoric India seem to have been dependent upon the land for food and therefore appear not to have been involved in food exchange with surrounding agricultural communities. The prehistoric case under examination was therefore probably not quite as specialized as the one indicated by Fox for the modern hunters and gatherers.

The scheme being outlined here suggests that the hunters and gatherers were the agents through whom the Harappan traders worked in the procurement of a broad range of raw materials useful to them in various manufacturing processes. The seasonal mobility of the hunter-gatherers would, through the course of a year, take them to or near the localities of materials such as steatite, shell, carnelian and other semi-precious stones. Copper ore may or may not have been procured in this fashion. It is difficult to believe that the hunter-gatherers themselves had a technology sufficiently sophisticated to take the ore and produce even crude, impure copper ingots. It is equally difficult to imagine them carrying the ore itself from the sources in southern Rajasthan. The exact nature of the procurement process utilized for this item is therefore left somewhat open and a topic for future research. What is clear, however, is that the hunters and gatherers whose life was closely tied to seasonal movement and who would have possessed an intimate knowledge of the natural resources within their range of movement can be envisioned as ideal groups with whom the Harappans could work.

It will be useful at this time to diverge from the main stream of discussion for a consideration of ancient trade in a broader context. This will serve to clarify some of the subtleties of the specific relationship under discussion.

Trade should be broadly defined as: "A reciprocal exchange or movement of materials or goods through human agency" (Renfrew 1969:152). It may be, in fact, that the term "exchange" is better suited to the "trading" situations of antiquity in that it carries fewer preconceived notions such as market and specialists. Such systems must have involved extremely complex activities and ties to various institutions. They were certainly not just economic arrangements. Recent studies of ancient trade, which were significantly anticipated in the work of Karl Polanyi and his collaborators (Polanyi, Arensberg and Pearson, 1957),

have come to see this as a varied phenomenon which may lack many of the features found in the modern marketing situation. For example, Polanyi suggests that markets themselves were frequently not present in antiquity. Trade was instead administered by a governing authority which moderated economic intercourse between institutions. Colin Renfrew convincingly argues that full-time traders, those middle men who derive their subsistence from trade and trade alone, may be a relatively recent innovation (Renfrew 1969:152). Owen Lattimore has also made an important point in his writing:

> There is danger in assuming that trade in the times in which (the historian) deals can be defined in the same way as trade after the rise of machine industry, capitalism, and modern banking. It was often a form of tribute, in part an exchange of luxuries rather than necessities, among ruling princes and great nobles, and in part, in transport and sale of grain from agricultural regions to pastoral nomads, it contributed to symbiosis in times of peace, between the two kinds of society (Lattimore 1940: xiii).

Thus ancient trade must at times be seen as restricted to a class of society, as an expression of a sociocultural relationship between two or more groups, and as symbiotic in both the natural and social senses. Trade could be used to reaffirm or validate existing sociocultural relationships. It was a symbol of good will. It might also establish a relationship of obligations between the participants which could be activated in times of need. In ecological terms it served to "spread the risk." If a group was faced with economic or political hardship, the pattern of trade often led to those upon whom one could call for support. It is in this regard that we can see the sociocultural advantages inherent in exchange relationships, even between two "kinds" of communities. Because these relationships are broadly integrative and reciprocal, and probably so at several levels, they should be seen as powerful adaptive mechanisms which foster cooperation, promote change and contribute to the survival of the participating communities each of which can be seen as occupying unique positions within a natural and sociocultural landscape.

The relationship between the inhabitants of Lothal and the hunter-gatherer communities in North Gujarat essentially describes a local phenomenon. But Lothal was, after all, only one settlement within a much larger civilization, and at some point its context within this system ought to be established. First to be noted in this regard is that Lothal is not a central place in the sense that a locational analyst would use the term. It is located on the south-eastern periphery of the Indus Civilization at that point which separates this urban system from the further reaches of western India. Second, it has been established that the town was an important place for the acquisition of a rather diverse inventory of raw materials such as copper (ore?), steatite, carnelian, shell, ivory and other items enumerated above. Third, none of these raw materials is available within either Sind or the Punjab and yet have been found in quantity at places such as Mohenjodaro and Harappa, and therefore must have been in considerable demand there. Lothal thus emerges as a logical intermediary through which Harappan entrepreneurs gained access to a broad range of products not available in the greater Indus Valley. Much of this raw material must have simply passed through Lothal enroute to the more central reaches of the civilization. The relatively direct overland route across the Rann of Kutch into southeastern Sind and then on to the Indus itself would seem to have been used judging from the recent discovery of several Harappan sites in Kutch (Joshi 1974) and the known distribution of sites in Pakistan. The sea route down the Gulf of Cambay to the Arabian Sea and then to the Indus River may have been used as well, but whether or not any goods left Lothal and entered directly into the often suggested intercivilizational maritime trade has yet to be established (Oppenheim 1954; Rao 1973:114-26).

In contrast to the pattern established for the distribution of raw materials from Lothal to major urban centers of the Indus Civilization, the manufacturing of finished products attested at the site is probably best seen within the parochial context of localized relationships. It is reasonable to project that a portion of the raw materials moving through the site would have been diverted for local consumption, or turned into finished trade goods and thus put back into the system of exchange. It is conceivable that some of the finished work may have been directed back to Sind, but considering the fact that Lothal is a site of modest size, unmistakably located on the border of the civilization some three hundred miles from Mohenjodaro, it seems an unlikely spot to have been a key center of craftsmanship for the larger urban centers. It simply makes better sense to see the craftsmen at Lothal as directing their efforts toward the creation of what were

essentially trade goods. A sound examination of Indus material in terms of defining local styles and their distribution would, however, be a means of testing this assumption.

Lothal, then, stands as a contrast to most other Indus settlements such as Chanhudaro, Judeirjodaro, Jalilpur and the two cities. These latter sites all seem to have been central places of the type referred to by Christaller (1933) and Losch (1938, 1954). They sit at or near the center of a regular, more-or-less homogeneous settlement grid. Clearly, Lothal was not a central place but rather a kind of gateway between the civilization and an area outside its bounds.

Gateway is a useful term in geography. It offers the user a range of analogies in that it implies both entrance and passage. The term has a dynamic quality in that it suggests physical movement and transportation. But it is also static in that such features are generally points of opening along some closed feature, such as a wall or border. Gateway settlements incorporate these features.

> Gateway cities (and towns) often develop in the contact zones between areas of differing intensities or types of production, along or near economic shear lines. They tend to be between differing homogeneous regions (Burghardt 1971:270).

Because they are settlements they are static points, through which persons and goods move. They are therefore characteristically geared to transportation. Just as a gateway may be enlarged to take care of increased traffic, so too does the settlement grow and respond to such stimulus.

Gateway settlements can be contrasted to central places.

> Perhaps the most obvious contrast would appear to lie in the shape of the city's service area. The central place, by definition, is located towards the center of its tributary area, whereas the gateway city is located eccentrically toward one end (Burghardt 1971:269-70).

The service area for places of this kind is most often fan or funnel-shaped and asymmetrically extended away from the parent core area. The maximum "range" of goods and services offered by the gateway settlement is vastly expanded to one side of the border on which it is located.

This geographical model has some utility with respect to our interpretation of Lothal. The notion of the gateway settlement lying on a border or frontier linking different homogeneous regions seems viable. Lothal would appear to have been a kind of "point interface" between the hunting and gathering peoples in North Gujarat and its parent Harappan hinterland. If judged from the data so far generated, its service area for finished goods could extend nowhere except into North Gujarat. The nonsymmetrical quality of this geographical feature is therefore supported. The "pioneer" quality of Lothal fits well with this model. It appeared suddenly and was, after all, distantly removed from any settlement with which it might be compared. Lothal can thus be imagined as one gateway through which Indus traders had access to the material richness of western India. For their part the hunting and gathering population, with whom these traders dealt, gained access to a variety of goods and services not previously available to them.

This discussion, then, suggests that Lothal was an entrepôt, a settlement of entrepreneurs interested in the procurement of raw materials demanded by the inhabitants of the cities and towns of Sind and the Punjab. To facilitate this process of acquisition they formed an alliance, an exchange relationship, with the hunting and gathering peoples on the southeastern borderland of their civilization. Seasonal mobility plus an intimate knowledge of the resources available within the regional environment made these hunter-gatherers efficient partners in the overall acquisition process. But there is more than merely culture historical importance to this relationship. What it suggests is that within the late third and early second millennia B.C. a complex interlocking cultural mosaic was developing in South Asia. This mosaic is not one which only portrays cultural diversity within simple geographic bounds and for which border phenomena and territoriality are particularly useful in terms of complete analysis. The mosaic is rather one which suggests a growth of interdependence between sociocultural groups with fundamentally different systems of settlement and subsistence, material culture and presumably diverse cultural traditions. This is a form of cultural integration still found in India as revealed by the ethnographic analogy employed for the hunter-gatherers. This interlocking relationship is also generally applicable to one of the essential aspects of caste organization if viewed as a total system. That is general economic integration of what are in deep

historical terms ethnically diverse populations. Without making the undue claim that the discussion of the relationship between the Harappan Civilization and hunter-gatherer groups in Gujarat charts the beginning of the caste system it can nonetheless be suggested that the development of a complex and integrated cultural mosaic has its roots in prehistory and that such conditions are wholly congruent as a precondition to the growth of caste institutions in later historical contexts. This is, in fact, the larger significance of the discussion at hand, and one which deserves a greater attention in archaeological research.

NOTE

[1]Radiocarbon dates will be given in two forms. The dates with capital "B.C." are those based on the 5730 half-life, those in lower case "b.c" have been corrected by tree-ring calibration (see Ralph, Han and Michael 1973).

Part VII The Environment

Editor's Introduction

The environment and cultural ecology of the Indus Civilization has already received considerable attention in several of the papers which appeared in earlier parts of this book (see especially Fairservis 1961 and 1967). There is a major issue, however, which deserves more extensive review because of its potential importance. This is the debate concerning climatic change and its effect on the Indus population.

Based on the work pioneered by Sir Aurel Stein (1931) and the writings of V. Gordon Childe, it was once thought that all of the Near East and the western parts of South Asia had been subjected to severe post-pleistocene desiccation. The presence of stone dams called *gabarbands* and large numbers of prehistoric archaeological sites in areas now almost devoid of settled peoples seemed to fit such an hypothesis. Robert Raikes and Robert Dyson, in the paper reprinted here, offer us an evenhanded review of this evidence and then go on to critically challenge the earlier conclusions. They feel that the degraded environment in these regions is more probably due to man's over-exploitation than to variation in rainfall and temperature regimes. However, Gurdip Singh's "The Indus Valley Culture" and the little read paper by C. Ramaswamy, both of which appeared after the Raikes and Dyson article, attempt to provide the hard data necessary for clear palaeoclimatic reconstruction.

Singh, interpreting pollen from Rajasthan lakes and Ramaswamy working from an important study by Lamb, Lewis and Woodruff (1966) both suggest that there was a period of higher rainfall in Pakistan and western India in the 2000-3000 B.C. time range. An interesting coincidence of agreement from two different lines of reasoning. They part company however, when they come to the duration of this period of relative wetness. Singh's data indicate that a period of desiccation set in at about 1800 B.C. whereas Ramaswamy reasons that it did not occur until approximately 500 B.C. If Singh is correct, climatic change could have been a factor in the abandonment of Mohenjo daro, Harappa and some other Indus settlements.

The abandonment of Mohenjo daro and Harappa at approximately 1900 B.C. is apparently an incontrovertible historical fact. But many other Harappan sites seem to have survived, even prospered, through the same period during which the two cities were abandoned. For example, the very substantial Ochre Colored Pottery complex in the Punjab has clear continuity through the Urban and Post-urban Phases of the Indus Civilization (Possehl 1974:32-36). If there was desiccation at about 1800 B.C. it would be reasonable to expect that there would have been a general decrease in the number of settled village farming communities in the Punjab and even Gujarat. But the archaeological data would seem to indicate just the opposite. In the Punjab, especially the east, and in Gujarat, there is an increase in the number of settlements after 1800 B.C. and this is a fact with which Singh has not dealt, and it may suggest that his interpretation of the variations in frequency of certain key plants is incorrect.

An alternative explanation for these pollen fluctuations is suggested from the work of R.L. Raikes (1968a) at Kalibangan. He has convincingly argued that the now dry Gaggar-Hakra river system of northern Rajasthan carried a substantial flow of water when that site was occupied. He further demonstrated that the river system dried up not due to smaller amounts of precipitation but rather because of tectonic activity in the northern Punjab which altered the head water drainage pattern and caused water which once

flowed out of the Himalayas to the west, to join the Ganges system and hence flow to the east. If similar events affected Singh's Rajasthan oases the variation in pollen could be due to changes in the amount of water entering the lakes, but not to a change in the regional rainfall pattern. Clearly there is much yet to be learned about this aspect of South Asian archaeology.

The Prehistoric Climate of Baluchistan and the Indus Valley

ROBERT L. RAIKES and ROBERT H. DYSON, Jr.

The late Aurel Stein made archeological observations which led him to believe that there has been a significant decrease in the rainfall of Baluchistan since prehistoric times due to a change in climate (Stein 1931).[1] This belief has been widely accepted and forms the background for Sir John Marshall's interpretation of the remains of the Indus Civilization (Marshall 1931:2). The views of these two scholars have been repeated by later authors (Vats 1940:4; Piggott 1950:134-135; Wheeler 1953:5-6; Spate 1957:145), so that the established position on the subject in archeological literature put in its mildest form is that "... a certain degree of climatic change is beyond dispute ..." (Wheeler 1953:6).

The degree of certainty which may be granted to this assumption depends, first, upon the validity of the individual inferences drawn from each category of evidence, and secondly, upon the extent to which such inferences lead to a common interpretation. Such a process of reasoning is necessary for the reconstruction of environment, which in turn is necessary to the full understanding of ancient cultures. It is, however, a process beset with difficulty and opportunity for error, and the conclusions reached should be subjected to constant critical review in the light of the available evidence. The lack of such a critical review in regard to the assertions made about ancient climate in the Baluchistan-Indus area makes this paper appropriate at the present time.

Let us begin with the arguments and evidence presented in support of the hypothesis of desiccation in Baluchistan. There are three major items: (1) the presence of *gabar-bands* (stone-faced embankments or "dams" attributed by the Baluchis to the *gabar* or Zoroastrians, intended for the control of surface water, and usually presumed to be prehistoric); (2) the location and number of prehistoric mounds in contrast to present-day settlements; and (3) the depth of the mounds and the sharply defined "cultural" areas, indicative of cultural stability and lack of movement (Stein 1931:9, 13, 34; Marshall 1931:7; Piggott 1950:68-70; Wheeler 1953:5-6).

The presence of gabar-bands, considered in some cases to be storage dams (Stein 1931:7, 24, 145-147, 164, 168; Marshall 1931:7), is said to imply a supply of rainfall sufficient to justify their construction, yet precarious enough to necessitate storage. It is also inferred that their massive stone construction points to a large settled population as the source of the labor. But are these inferences valid? Storage dams are built to conserve inadequate rainfall. But were these structures storage dams? Probably not. Their capacities are negligible. Apart from this, most appear to be situated on porous gravel fans where storage for more than a few hours, or at most a day or two, would have been impossible. They seem much more likely to have been a means, quite often successful, of creating cultivable land—check dams in fact. Water probably passed through and under, and perhaps sometimes over, them but was held long enough for silt to be deposited gradually upstream. In many cases the silt on the upstream side is level with the top of the gabar-band, and in one instance Stein reported that this silt still showed traces of former cultivation (Stein 1931:24; cf. Bowen 1958 for southern Arabian silt formation). Agriculture in Baluchistan at the present time is carried on in "... larger fields surrounded by embankments three or four feet high, by which rainfall is

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caught as it descends from the gravel slopes bordering the valleys" (Imperial Gazeteer 1908a:294). Near Ughar, walls descend the slope parallel to each other. "The southern of the two . . . at its lower end serves as part of the enclosures which bound three successive cultivation terraces. Each of these contains a flat space of arable soil" (Stein 1931:147). In the Nal area north of Shakar Khan Damb, Stein reported a few fields "with a modern earth dam intended to catch for their benefit the drainage from a low ridge to the west. Parallel to this dam runs a ruined *gabar-band* which . . . obviously served the same purpose" (Stein 1931:168). In other cases gabar-bands seem to have been simple diversion structures intended to deflect some or all of a flood onto whatever soil was available before the water was absorbed into the gravel (Stein 1931:145-146). They were, in such cases, what would now be called water-spreading structures. Such structures are used today mainly under arid, not wet, conditions. There is, therefore, little sound evidence to support the assertion that gabar-bands were storage dams operating under wetter conditions.

The use of large quantities of manpower in the construction of gabar-bands may also be questioned. The massive blocks of which they are built seem evidence rather of engineering skill than of abundant manpower. Two typical blocks measured by Stein would have weighed one ton and two tons respectively, and, because of their small size, would have required rollers or levers and ropes rather than an army of men to lift or move them. Petrie has pointed out that in building the Gizeh pyramids a gang of eight men would probably have been sufficient to handle a single average stone block weighing about two and a half tons (Edwards 1947:229). In any case the very small area of cultivation made available near the gabar-bands could not have supported a very large settled population. Laborers, therefore, whether numerous or not, probably were imported for the purpose, perhaps as seasonal migrants on the present-day pattern. Certainly the size of the stone blocks need not argue for a large settled population in the vicinity.

Another major point is that the date of the construction of the gabar-bands is quite unknown. It is very possible that they are not prehistoric at all. In at least one instance the only pottery found in the area was medieval Islamic (Stein 1931:147). In Afghanistan similar stone structures are often attributed by local tradition to the Islamic period (personal communication from M. Raoul Curiel). Elaborate stone-built irrigation structures in South Arabia which are in some ways comparable are dated mainly to the first millennium B.C. (Bowen 1958:64-68). A date in historical times would hardly be an argument for a prehistoric rainy period; quite the contrary, it would show that quite recently there had been agriculture in the areas under discussion. Until the question of this dating can be resolved the presence of these structures in itself cannot be an adequate indication of the state of weather in antiquity.

The abandonment of the gabar-band system may have been due to events other than desiccation, such as the collapse of political control in the area or intrusions of outside groups with differing technologies. More important than these may have been the introduction of the Persian *karez* or *qanat*, a means of tapping ground water by driving sub-horizontal tunnels into gravel fans to intercept the water table, a method still in use (Smith 1953). Such a new system may well have resulted in a gradual redistribution of population through changed needs without reference to any change in rainfall. In the long run extensive use of the qanat may also have reduced the level of the water table itself.

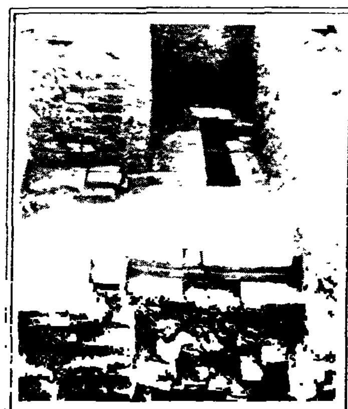
Another approach to the problem of climatic stability in Baluchistan lies in the comparative distribution of prehistoric and present-day populations. Such a comparison raises the questions of where people lived, how many of them there were, and how they lived.

In some instances the present-day settlement pattern coincides with the archeological pattern; in others it does not. In regard to coincidental patterns it has been noted in the Quetta area that ". . . sites occur almost everywhere in the valley where fertile soil and water exist today, indicating that climatic conditions and the ecology of the modern Quetta valley are comparable to those of prehistoric times" (Fairservis 1956a:359). It remains then to extend such comparisons to other parts of Baluchistan in order to check this point. In this regard it seems highly significant that no sites have yet been identified in the Las Bela Plain, in the Hab Valley (at any rate south of Dareji), nor in the main area of the Kacchi Plain (between Sibi and Jacobabad). The various routes that connect these plains with the hills[2] have one point in common; namely, their lowland terminus occurs near the foot of the hills. Only in the case of the Khozdar-Kolachi River—Gaj River —Lower Sind route is the terminus both at the foot of the hills and in the Indus flood plain, so that around Amri a considerable fanning out was possible.

PLATE I. *Material recovered from Mohenjo daro in the first seasons of excavation by Sir John Marshall.*



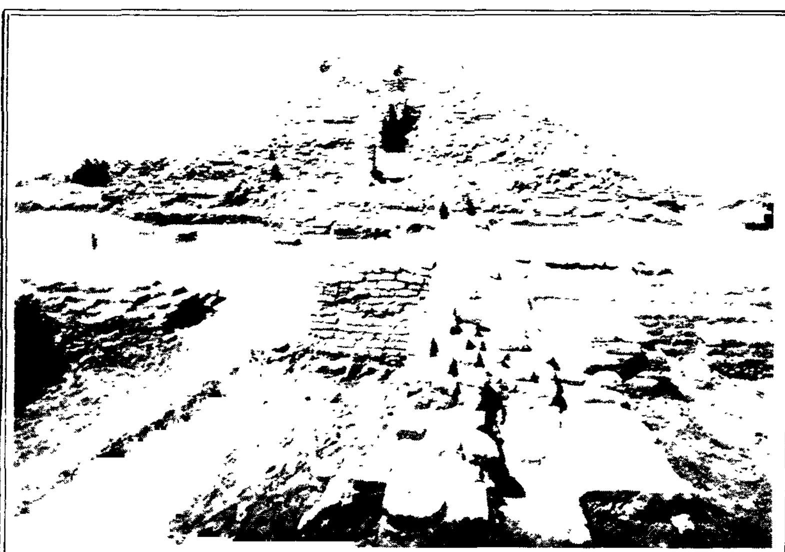
ONE OF THE TWO SITES WHOSE REMAINS ARE OLDER THAN ANYTHING YET KNOWN IN INDIA PREHISTORIC BUILDINGS EXCAVATED AT MOHENJO-DARO, SIND, DATING PROBABLY BETWEEN 1000 AND 4.0 B C



WITH FLOORING AND CONDUIT OF GLAZED BRICK A PREHISTORIC SHRINE AT MOHENJO-DARO



TO HOLD A HUNCHED BODY. A 3-FT. LONG BRICK GRAVE BUILT IN THE WALL OF A ROOM AT MOHENJO-DARO



BUILT MORE THAN 2000 YEARS AGO, BUT COVERING TWO STRATA OF EARLIER REMAINS THE SECOND-CENTURY BUDDHIST *STUPA* AT MOHENJO-DARO, ON AN ISLAND IN THE DRY RIVER-BED



SHOWING A POT (IN FOREGROUND) THAT CONTAINED EARLY INDIAN OBLONG "PUNCH-MARKED" COINS (*KARSHAPANAS*) MASSIVE BRICK WALLS OF THE SECOND AND THIRD PERIODS AT MOHENJO-DARO

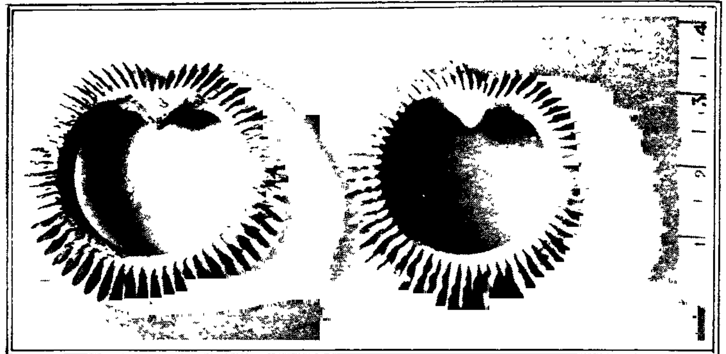


ON THE OTHER NEWLY DISCOVERED PREHISTORIC SITE IN INDIA, OCCUPIED FOR MANY HUNDREDS OF YEARS BEFORE THE THIRD CENTURY B C EXCAVATIONS AT HARAPPA, IN THE PANJAB

PLATE II. *Material recovered from Mohenjo daro in the first seasons of excavations by Sir John Marshall.*



FROM BALUCHISTAN, THROUGH WHICH THE DRAVIDIAN RACES PROBABLY ENTERED INDIA PREHISTORIC PAINTED POTTERY SIMILAR TO THE NEW DISCOVERIES



POSSIBLY WORN ON THE WRISTS OF PREHISTORIC INDIAN BEAUTIES SOME 3000 YEARS AGO BANGLES (ABOUT 3½ in ACROSS) OF BLUE GLASS PASTE, FOUND AT HARAPPA



TRINKETS WORN BY PREHISTORIC INDIAN PEOPLE MISCELLANEOUS BEADS OF CORNELIAN, SHELL, AND SO ON, FROM MOHENJO-DARO



USED IN PREHISTORIC URN-BURIAL TO HOLD FOOD OR RAIMENT AND PLACED WITH THE URN INSIDE A LARGER JAR MINIATURE FUNERAL POTTERY (1 TO 1½ IN HIGH) FROM MOHENJO-DARO.



INDICATING A HIGH DEGREE OF DECORATIVE ART IN THE PREHISTORIC PERIOD AT MOHENJO-DARO THREE FRAGMENTS OF POLYCHROME POTTERY WITH DESIGNS OF VARIOUS PATTERNS



BEAUTIFULLY SHAPED AND PROPORTIONED: A COMPLETE PAINTED VASE (ABOUT 6 IN HIGH) OF THE PREHISTORIC PERIOD FOUND DURING THE EXCAVATIONS AT HARAPPA



INDICATING AN ANALOGY BETWEEN THE PREHISTORIC ART OF THE INDUS VALLEY AND THAT OF BALUCHISTAN POLYCHROME POTTERY FROM THE LATTER COUNTRY (LOWER ROW) COMPARED WITH EXAMPLES FROM MOHENJO-DARO (TOP ROW)

PLATE III. *Material recovered from Mohenjo daro in the first seasons of excavations by Sir John Marshall.*



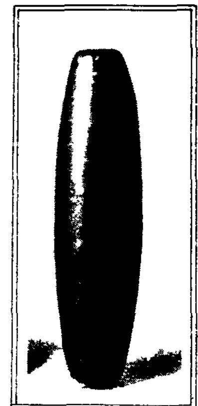
ENGRAVED WITH A PICTOGRAPHIC SCRIPT UNLIKE ANY PREVIOUSLY KNOWN INDIAN ALPHABET, BUT SOMEWHAT RESEMBLING MYCENÆAN PICTOGRAPHS PREHISTORIC SEALS FROM MOHENJO-DARO AND HARAPPA.



BEARING FIGURES OF BULLS AND MYSTERIOUS PICTOGRAPHIC SYMBOLS, SOME RESEMBLING ROMAN NUMERALS PREHISTORIC INDIAN SEALS FROM HARAPPA AND MOHENJO-DARO WITH AN UNKNOWN FORM OF PICTURE-WRITING



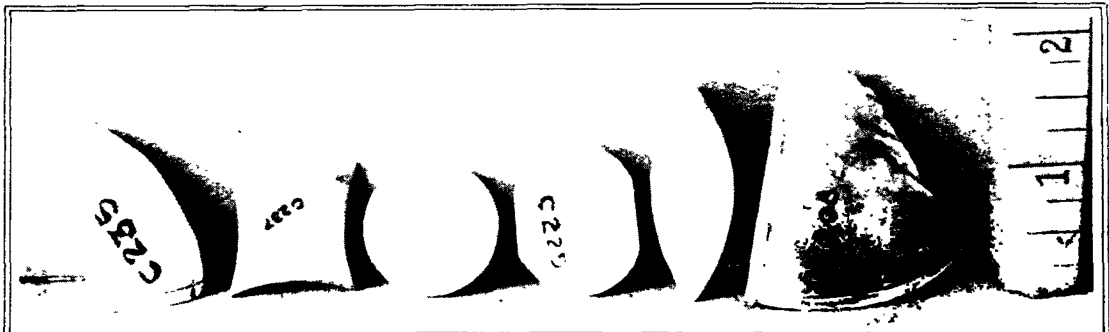
MADE PERHAPS TO AMUSE LITTLE PREHISTORIC PEOPLE IN THE INDUS VALLEY TWO OR THREE THOUSAND YEARS AGO TERRA-COTTA ANIMALS, BIRDS, AND OTHER TOYS FROM MOHENJO-DARO



USED WITH A PREHISTORIC MORTAR A PESTLE OF BLACK HÆMATITE

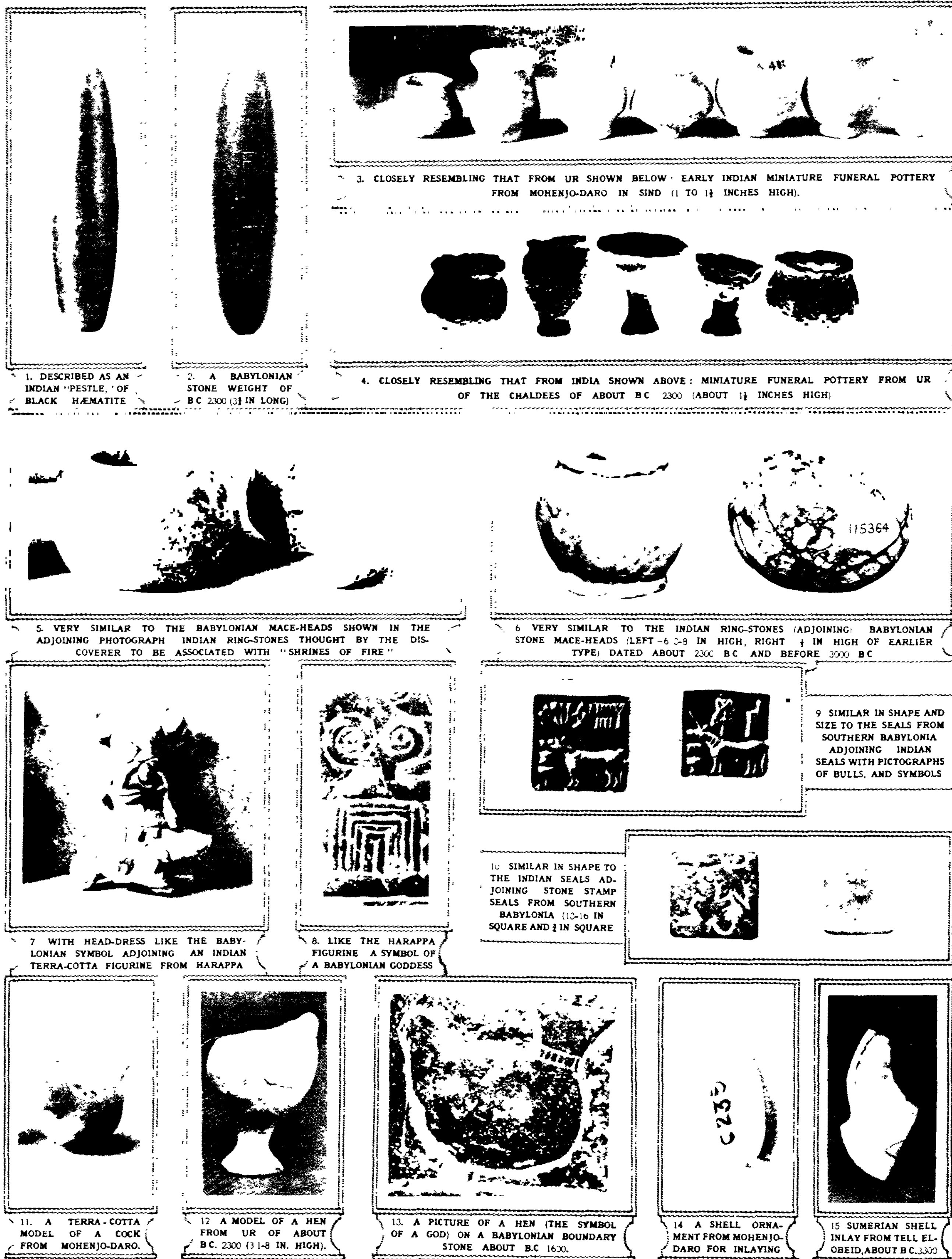


PREHISTORIC CREMATION A FUNERAL URN OF RED GLAZED WARE



PREHISTORIC INDIAN DECORATIVE ART MISCELLANEOUS ORNAMENTS OF CONCH SHELL FOR INLAYING, FOUND AT MOHENJO-DARO (WITH A MEASURE INDICATING THE HEIGHT OF THE BIGGEST ONE—2 INCHES)

PLATE IV. *Mesopotamian and Indus parallels noted by C.J. Gadd and Sidney Smith.*



1. DESCRIBED AS AN INDIAN "PESTLE," OF BLACK HÆMATITE

2. A BABYLONIAN STONE WEIGHT OF B C 2300 (3¼ IN LONG)

3. CLOSELY RESEMBLING THAT FROM UR SHOWN BELOW · EARLY INDIAN MINIATURE FUNERAL POTTERY FROM MOHENJO-DARO IN SIND (1 TO 1¼ INCHES HIGH).

4. CLOSELY RESEMBLING THAT FROM INDIA SHOWN ABOVE: MINIATURE FUNERAL POTTERY FROM UR OF THE CHALDEES OF ABOUT B C 2300 (ABOUT 1¼ INCHES HIGH)

5. VERY SIMILAR TO THE BABYLONIAN MACE-HEADS SHOWN IN THE ADJOINING PHOTOGRAPH INDIAN RING-STONES THOUGHT BY THE DISCOVERER TO BE ASSOCIATED WITH "SHRINES OF FIRE"

6. VERY SIMILAR TO THE INDIAN RING-STONES (ADJOINING) BABYLONIAN STONE MACE-HEADS (LEFT—6 3-8 IN HIGH, RIGHT ¼ IN HIGH OF EARLIER TYPE) DATED ABOUT 2300 B C AND BEFORE 3000 B C

7. WITH HEAD-DRESS LIKE THE BABYLONIAN SYMBOL ADJOINING AN INDIAN TERRA-COTTA FIGURINE FROM HARAPPA

8. LIKE THE HARAPPA FIGURINE A SYMBOL OF A BABYLONIAN GODDESS

9. SIMILAR IN SHAPE AND SIZE TO THE SEALS FROM SOUTHERN BABYLONIA ADJOINING INDIAN SEALS WITH PICTOGRAPHS OF BULLS, AND SYMBOLS

10. SIMILAR IN SHAPE TO THE INDIAN SEALS ADJOINING STONE STAMP SEALS FROM SOUTHERN BABYLONIA (13-16 IN SQUARE AND ¾ IN SQUARE)

11. A TERRA-COTTA MODEL OF A COCK FROM MOHENJO-DARO.

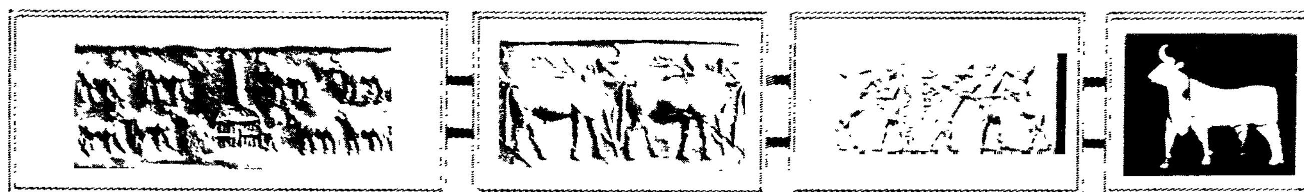
12. A MODEL OF A HEN FROM UR OF ABOUT B.C. 2300 (3 1-8 IN. HIGH).

13. A PICTURE OF A HEN (THE SYMBOL OF A GOD) ON A BABYLONIAN BOUNDARY STONE ABOUT B.C 1630.

14. A SHELL ORNAMENT FROM MOHENJO-DARO FOR INLAYING

15. SUMERIAN SHELL INLAY FROM TELL EL-OBEID, ABOUT B C.3300

PLATE V. *Mesopotamian and Indus parallels noted by C.J. Gadd and Sidney Smith.*



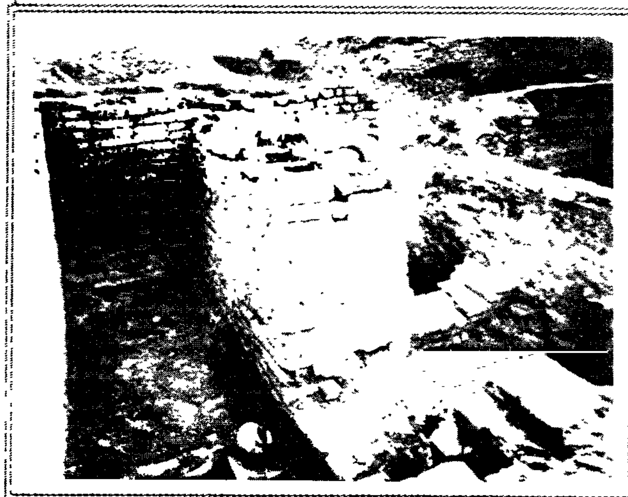
TO BE COMPARED WITH THE INDIAN PICTOGRAPHIC BULLS BELOW · IMPRESSIONS OF TWO CYLINDER SEALS IN THE LOUVRE SHOWING SUMERIAN REPRESENTATIONS OF BULLS ABOUT B C 3000.

WITH BULLS' NECKS AS IN THE INDIAN PICTOGRAPHS A BABYLONIAN SEAL ABOUT B C. 2000.

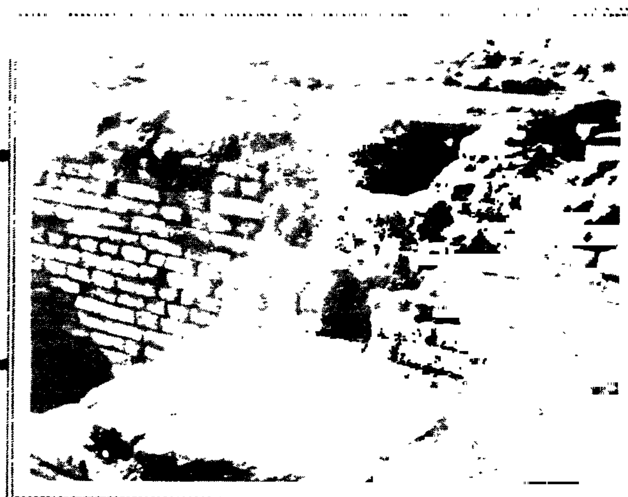
INLAY OF A BULL FROM TELL EL-OBEID B C 3300.



FOR COMPARISON WITH THE SUMERIAN REPRESENTATIONS OF BULLS SHOWN IN THE PHOTOGRAPHS ABOVE PREHISTORIC INDIAN SEALS FROM HARAPPA AND MOHENJO-DARO, WITH FIGURES OF BULLS SIMILARLY MARKED, AND SYMBOLS RESEMBLING THE SUMERIAN SCRIPT



SHOWING THE SIMILARITY OF THE INDIAN BRICKWORK TO THAT OF UR SEEN IN THE ADJOINING PHOTOGRAPH WALLS EXCAVATED AT MOHENJO-DARO, IN SIND



WITH BRICKWORK LIKE THAT FOUND IN INDIA (SEE ADJOINING PHOTOGRAPH) PART OF THE OUTER WALL OF UR, WITH A DRAIN OF BURNT BRICKS

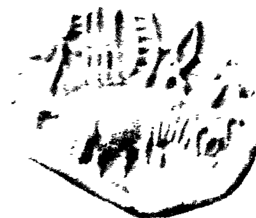


WITH A FLOOR AND CONDUIT OF GLAZED BRICKS RESEMBLING THOSE AT UR ADJOINING AN INDIAN SHRINE AT MOHENJO-DARO



PAVED WITH SIMILAR BRICKS AND HAVING A CONDUIT OR DRAIN (*B*) RESEMBLING THE INDIAN WORK SEEN IN THE ADJOINING PHOTOGRAPH A BABYLONIAN SANCTUARY AT UR (*A*) THE ALTAR OF BLOOD SACRIFICE, (*C*) THE UPPER COURT



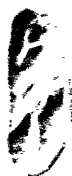


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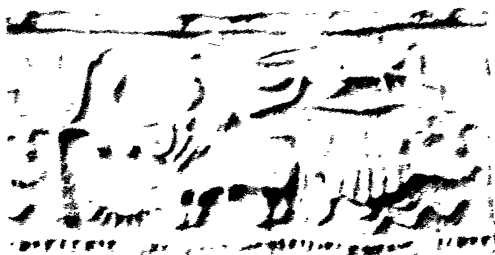

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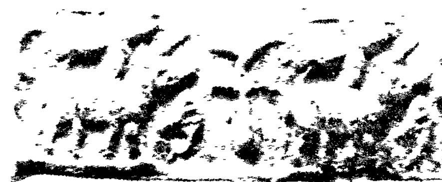





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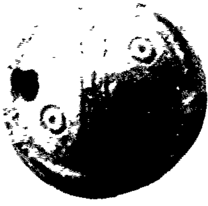
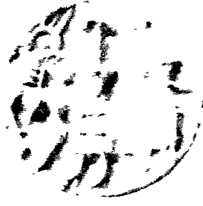


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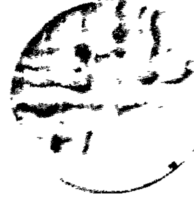
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SEALS OF INDIAN STYLE FROM UR

PLATE VII.

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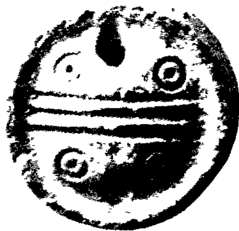
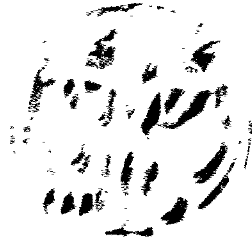
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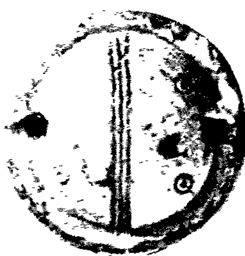
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SEALS OF INDIAN STYLE FROM UR







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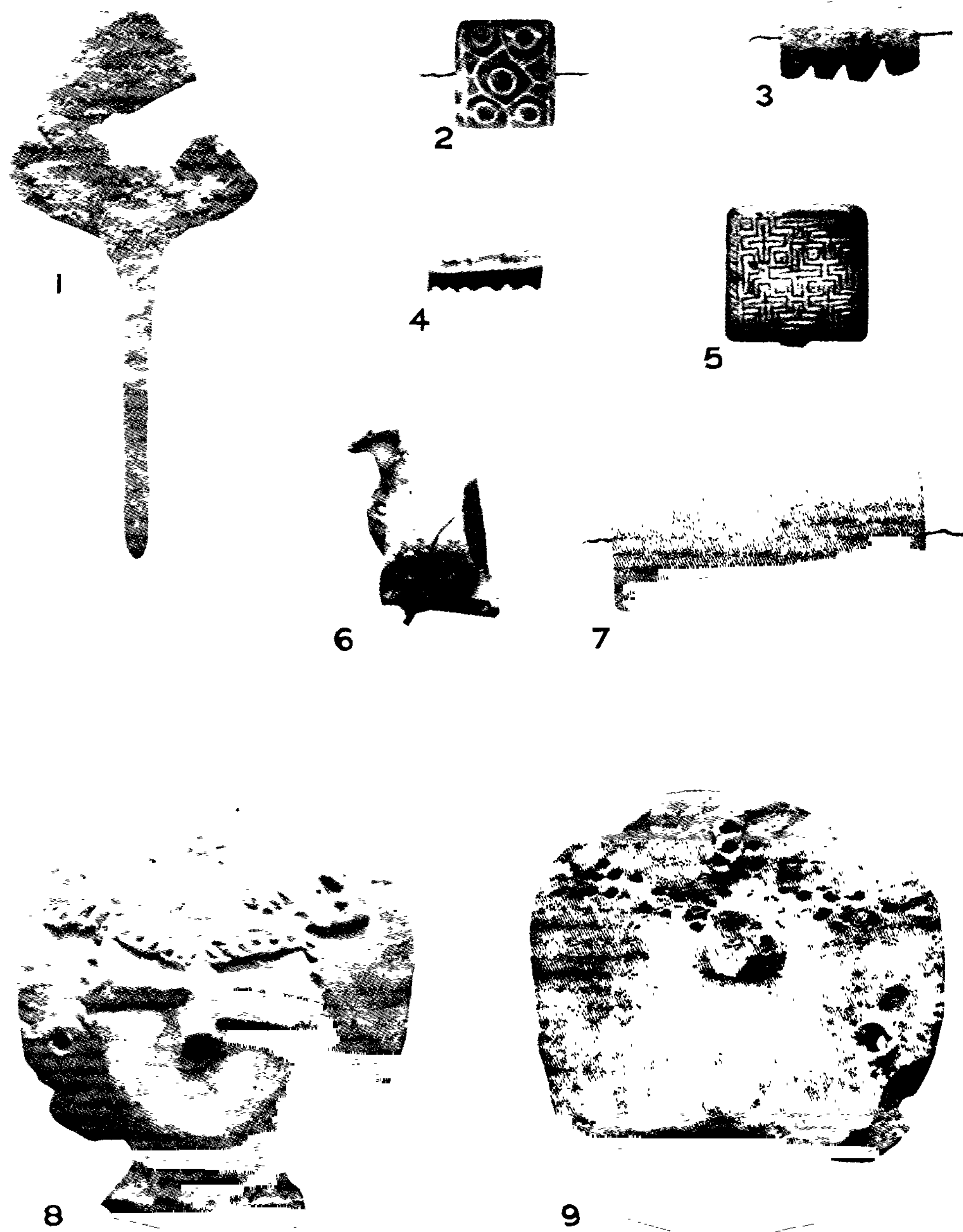




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SEALS OF INDIAN STYLE: UR AND BABYLONIA

PLATE IX.



1, Copper blade 2, 3, Beads, Mohenjo-daro 4, Sickle-flint 5, Silver Ring

6, Toy animal Kish 7, Rectangular bead, Mohenjo-daro 8, 9, Pottery heads, Kish

PLATE X. *The "Persian Gulf Seal" from Lothal.*
(a) *Obverse*, (b) *the cast*, (c) *reverse*
Scale · X 2.



(*a*)



(*b*)



(*c*)



PLATE XI. *Kalibangan, house remains of Period I.*



PLATE XII. *Kalibangan, part of the 1.5 meter wide lane of Period I. The house walls are only a single brick thick.*



PLATE XIII. *Kalibangan, characteristic pottery of Period I, mostly Fabric A.*

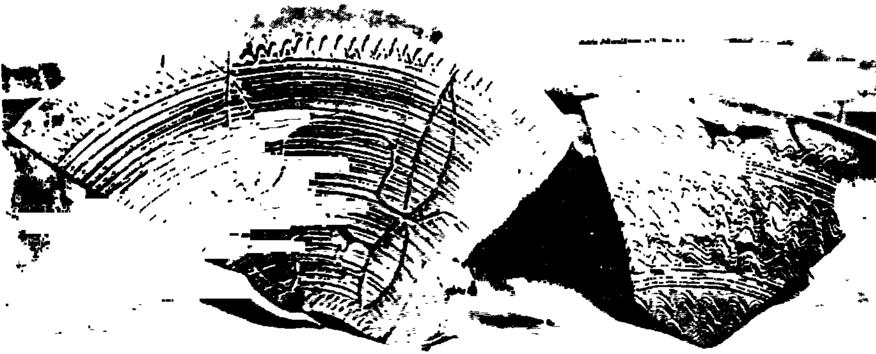


PLATE XIV. *Kalibangan, characteristic pottery of Period I, Fabric D.*

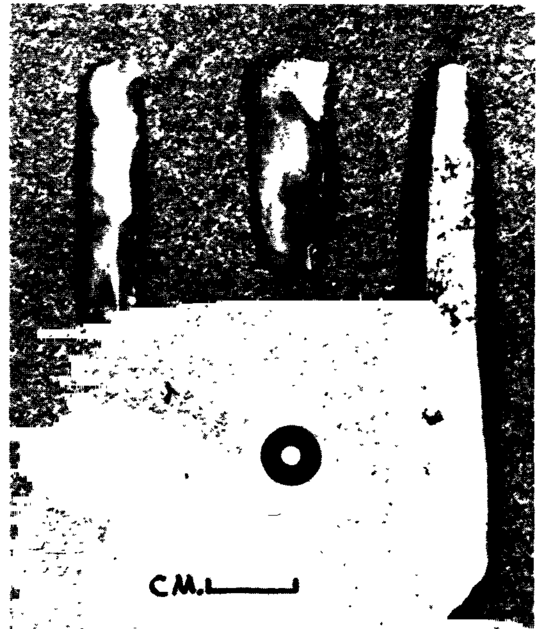


PLATE XV. *Kalibangan, miscellaneous objects including blades, beads and bone points from Period I*



PLATE XVI. *Kalibangan, Citadel area, rectangular salient projecting approximately 8 meters from the partition fortification wall and measuring 13 meters, Period II. Mark the taper obtained by a thick coat of plaster.*



PLATE XVII. *Kalibangan, Citadel area, row of fire altars on one of the mud brick platforms, signifying the performance of some ritual, Period II.*



PLATE XVIII. *Kalibangan, Citadel area, brick lined pit atop a mud-brick platform, containing bones of a bovine, antlers, etc., suggesting animal sacrifice, Period II.*



PLATE XIX. *Kalibangan, Citadel area, residential annex. View showing the excavated houses and a street, Period II.*



PLATE XX. *Kalibangan, Lower city, close view of an excavated north-south running street, Period II. The width of the street seems to have been maintained throughout the occupation of the site, speaking of the strict enforcement of civic laws.*

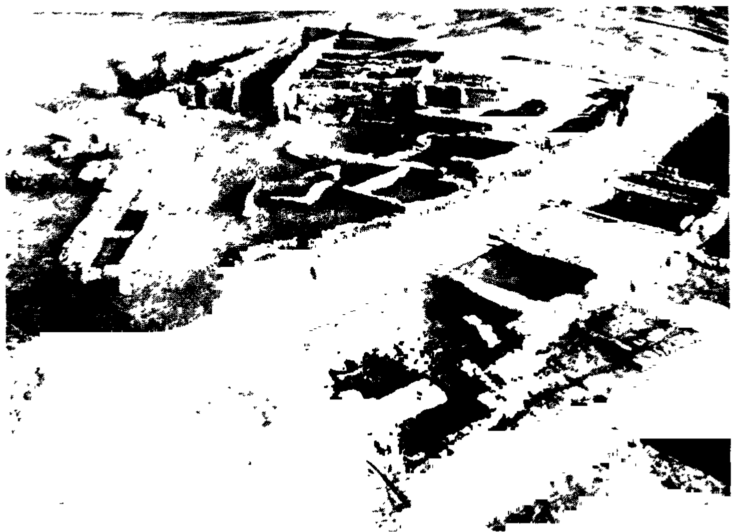


PLATE XXI. *Kalibangan, Lower city, general view of the two north-south running streets converging on the entrance situated in the northwestern corner of the city. Period II.*

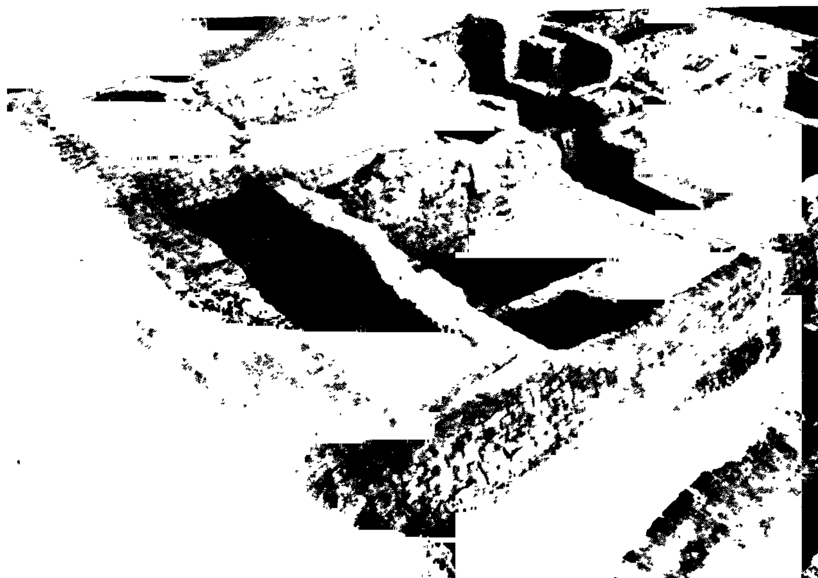


PLATE XXII. *Kalibangan, Lower city, an excavated house showing streets on three sides; the triangular area in the right hand corner represents part of the courtyard. Period II.*



PLATE XXIII. *Kalibangan, cylinder seal from Period II and its impression.*





PLATE XXIV. *Kalibangan, incised terra-cotta cake from Period II.*



PLATE XXV. *Kalibangan, copper bull from Period II.*

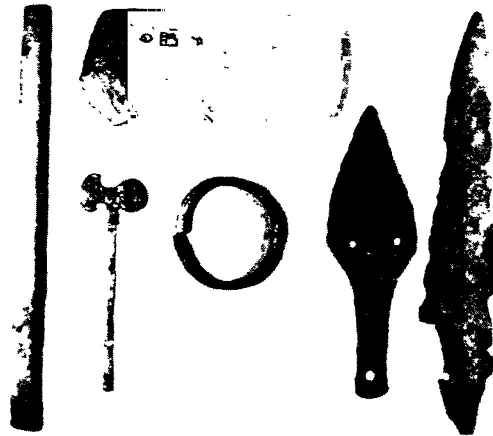


PLATE XXVI. *Kalibangan, typical copper objects, including a pin, from Period II.*

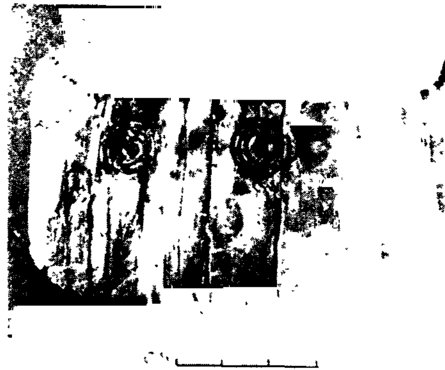


PLATE XXVII. *Kalibangan, ivory comb from Period II.*

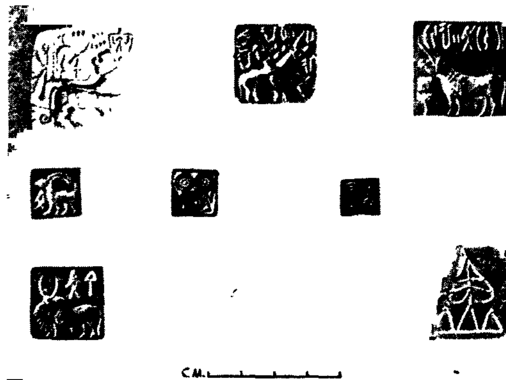


PLATE XXVIII. *Kalibangan, seals from Period II.*

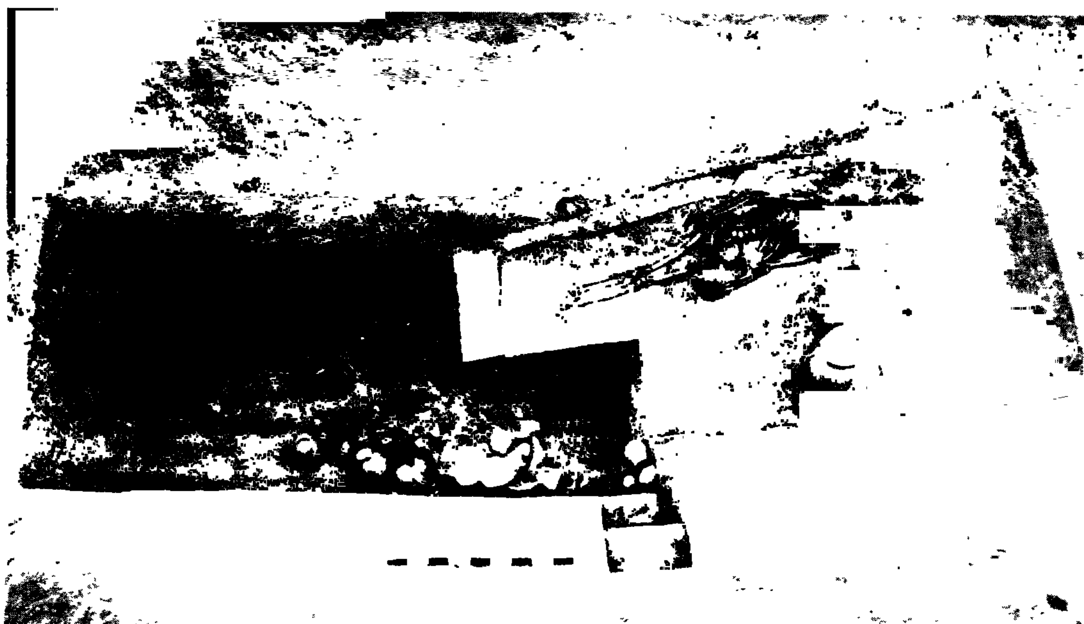


PLATE XXIX. *Kalibangan, cemetery with rectangular grave containing two interments, the lower being without any skeletal remains and the upper showing an extended human skeleton with the pottery deposit, Period II.*



PLATE XXX. *Kalibangan, cemetery with circular grave pit containing an urn along with other pottery. No skeletal remains were associated with this type of burial.*

In three other instances[3] the route termination is at the edge of the Kacchi Plain. This plain is one of the most inhospitable places on earth: an immense area of dead, flat, buff-colored mud stretching for 100 miles between Sibi and Jacobabad. It varies in width from about 30 miles at its northern end to about 80 miles at the southern end. Except for its periphery it is completely waterless, and apart from occasional low desert scrub and grass in the drainage lines it is completely bare. The temperature in June and July goes up to about 130° F. with an average day and night temperature in July of well over 100° F. If, in pre-historic times, the plain had been either just hot or just treeless (because waterless) people might have ventured out into it; but the combination would have meant suicide. The location of sites in this plain area is, therefore, an important piece of evidence as to whether or not such was the case. To date only a few sites are reported from the northern end of the plain; one just south of Sibi,[4] one (late prehistoric) near Dadhar, one (late prehistoric) north of Sibi, and some near Shoran. At the southern end of the plain there is a new site of the Harappa culture, Judeir-ko Damb,[5] about 20 miles north of Jacobabad. This is the northernmost Harappa site in the area, located at a point where tamarisk could grow today were it left undisturbed by goats and camels. Further north the water table drops below the reach of primitive wells or tree roots. The absence of sites in the central area would thus appear to indicate that both for the hillsmen and the Harappans the Kacchi Plain in 2000 B.C. was not much different from its present uninviting self.

The apparent avoidance by prehistoric people of the Las Bela Plain and the lower Hab River is harder to explain. Edith Shahr, a site of the Harappa culture, has recently been discovered just north of Bela at the head of the Las Bela Plain on the Porali River. At first sight it would seem to have been on a line of communication stretching from Ninglaj at the mouth of the Hingol River through Bela to Wad, as there do not seem to be any known sites along the coastal stretch via Karachi and the Indus mouth to Amri. In this sense it would form part of a communications route between the sea coast and the interior—a pattern which seems also indicated by the Harappan outposts recently visited by a University of Pennsylvania Museum expedition at Sutkagen-Dor and Sotak-koh further west along the Makran coast. On the other hand, it is not altogether far-fetched to see it as the southern end of a line of communication from Wad, the latter being reached via the Kolachi and Gaj Rivers from Amri, or via the Mula River from Mohenjo Daro and Jodeir-jo-daro. Clearly in a discussion of this problem, military and trading frontier posts of the Harappa culture, supported by the culture of the main Indus valley area, will have to be differentiated from sites representing local, possibly transhumant, cultures based upon available indigenous resources. In the case of the lower Hab River and the Las Bela Plain it may be that further study will reveal additional sites. At present, although the inhabitants know what an archeological mound (or *damb*) is, they have not reported any. Mounds exist south of Khozdar, around Wadh, and at the head of the Kud River, but, excepting Edith Shahr, they are unknown beyond the escarpment. One possible explanation of this apparent emptiness lies in the fact that the Hab River valley is hot and uninviting except to herdsmen and is said to be a very difficult route, while the Las Bela Plain formerly may have been a tidal marsh which has since silted up.

To evaluate conclusions based on the presence of ancient sites in areas not now supporting a large settled population it is necessary to consider the actual size of the older population and the cultural pattern followed by people in the area then and today. When Stein reports that "the total absence of a settled population either in Besema or anywhere else within a radius of thirty miles . . . served fully to bring home to me the great change which has come over this region since prehistoric times" (Stein 1931:32-34), the argument sounds convincing. Yet a careful reading of the text readily shows that this same area was not only inhabited at the time of writing but also cultivated—*although not by settled people*. Such a discrepancy requires careful examination.

Population size in ancient times must be inferred from the size and number of contemporary sites and burial grounds. Piggott has previously pointed out that the largest mound of the Quetta group can never have been more than a small village, while the average size of the Amri-Nal settlements did not exceed two acres (Piggott 1950:73, 77; for distribution maps of sites see Piggott 1950: Fig. 2; Wheeler 1953: Figs. 2 and 3). Given such small village units in antiquity, it is necessary to know how many of them were occupied at any one time. This does not mean within a major period of a century or two, but within say a period of 50 years. Such a chronology does not exist. Indeed, no chronology soundly based on stratigraphic excavation yet exists in this area, with the exception of the work of

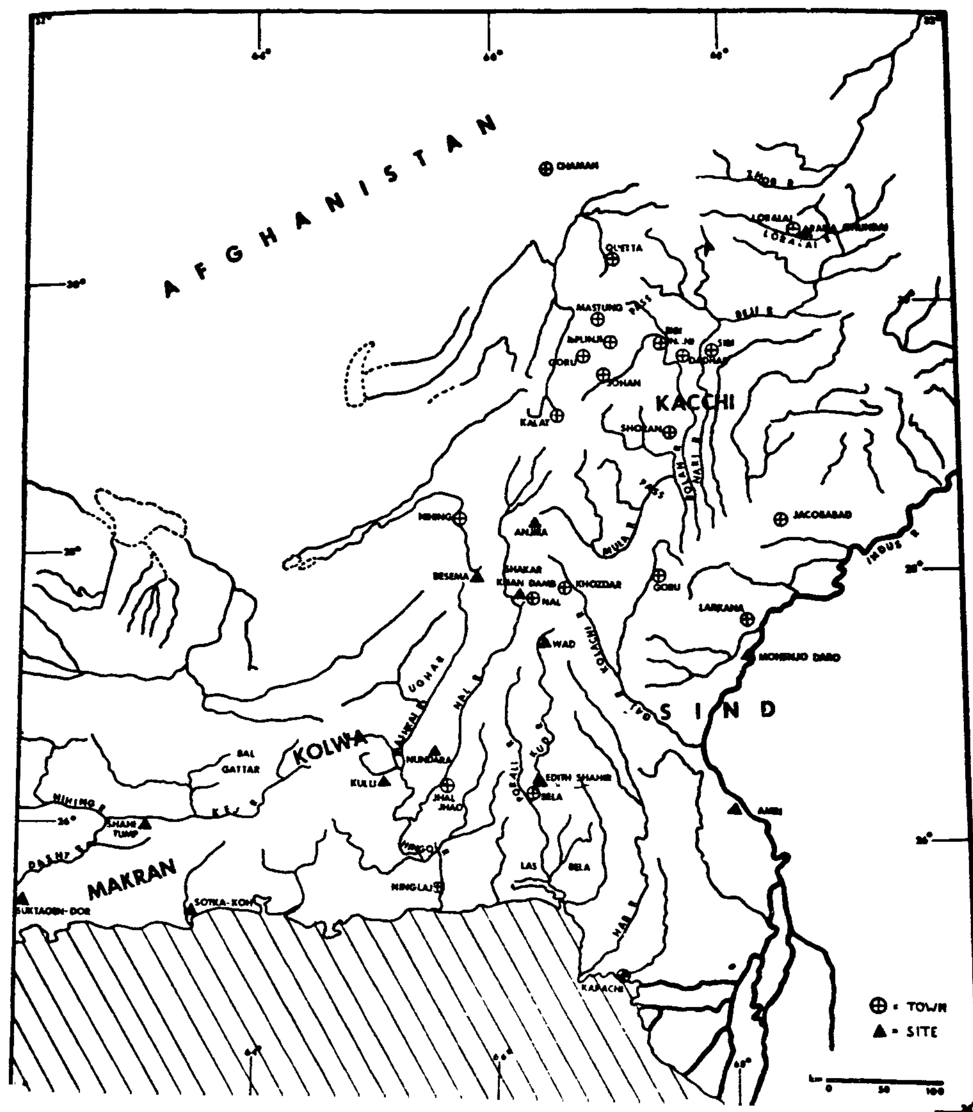


FIGURE 28.1. *Map showing towns and sites in the Lower Indus Valley and Baluchistan.*

Fairservis (1956a) and De Cardi (1959). A beginning in stratigraphic exploration has been made but is as yet insufficient to the chronological needs as stated. Nor is there sufficient data from burials to aid in any significant way. The 100 estimated burials recovered from the cemetery at Nal (Piggott 1950:81) lack stratigraphic context. It is also possible that they represent a communal burial area serving more than one village. Clearly, until local sites can be adequately mapped with soil and rainfall data against a reliable chronology, no very substantial conclusions may be drawn from them.

Furthermore, the significance of these sites must be viewed in terms of the cultural pattern which they may represent. The dependence of cultural patterns in arid and semi-arid countries on the climate needs no stressing to those who have lived and worked in such areas. To others such dependence may be less obvious. The way of life of the present-day inhabitants of Baluchistan is almost entirely conditioned by the water supply. Whether the people are Baluchis, Brahuis, Pathans, or Kurds makes no difference. All are dependent on primitive flood irrigation (*sailaba*), dry farming (*khushkaba*), and such scanty water as can be obtained from wells, springs, small perennial streams, and *karezes*. All live in the same kinds of houses, all use

more or less the same agricultural methods. All, except those fortunate enough to have both perennial water and a mild winter climate, migrate seasonally. They must do this in order to keep warm, to find grazing for their flocks, and to exchange their labor for food in the plains. If in prehistoric times the climate was the same as that of today, then the prehistoric people may well have followed a pattern similar to that of today. This means they would have migrated seasonally in order to survive, a possibility already noted by De Cardi in reference to the archeology of the Bolan and Mula Pass areas (De Cardi 1959:23). Should such local transhumance be assumed, the effect on the interpretation of archeological remains in the area would be profound. It would be difficult to believe in a number of isolated but contemporary cultures—as at present inferred from pottery styles (Zhob, Quetta, Nal, Kulli, etc.). It would be more likely that there was a complex of small tribes distributed in a mosaic pattern and much concerned then, as now, with local loyalties. They would be the ancient parallels to the modern Baluchis, Brahuis, Pathans, etc., probably following tribal and familial traditions in their pottery making. Such a picture might account for the wide scatter and intermingling of pottery types (as shown by existing distribution maps) which makes the present area designations a little misleading. Under modern conditions of seasonal migration, many tribes have complementary headquarters—a summer one in the hills, and a winter one on the Kacchi Plain for instance. Even if a *Sirdar* (chief) himself does not migrate (owing to his having a warm house in Quetta or Mastung), members of his family do. The counterparts of the Sirdars in prehistoric times would have been the sort of families that owned fine decorated pottery in both their houses—thus producing double the number of permanent settlements actually used by the population and a partially localized culture in periodic contact with other groups.

Until recently the only positive evidence of any kind of a connection between hills and plains was provided by the Amri-Nal complex of sites and by the existence of Indus Civilization artifacts at some hill sites. To this evidence may now be added that of two new sites.[6] One, at Dranjan Levy Post in the Lower Bolan Pass, is tentatively identified by members of the Department of Archaeology of Pakistan as a settlement of the Quetta culture with a possible subsequent Harappan occupation. The second site is a fairly large mound near Pirak abandoned railway station, 10 miles south of Sibi. This mound is 200 yards long (north to south), about 120 yards wide, and 8 yards high. The pottery has been tentatively identified as "Quetta" ware. There are also similarities to Shahi Tump (Makran) pottery. Two clay button-seals from the site are almost identical with Shahi Tump specimens. There also seem to be similarities with Zhob pottery. A few pieces of what Fairservis calls "Faiz Mohammad Gray Ware"[7] and other traces of later occupation have been recognized. These two sites on the plain, therefore, show connections with two or three other "cultures" in the hills. When it is realized that this area of the northern Kacchi Plain is today the winter terminus of several seasonal migration routes (including the Quetta—Kacchi and Loralai—Kacchi routes) it is perhaps not out of place to question the significance of these relative locations and contacts and to leave the problem of cultural pattern in antiquity open to further debate.

It has been said that the depth per se of some of these mounds indicates cultural stability in the area—a stability which in turn is said to reflect an increased rainfall in antiquity. Aside from the problem of possible migratory patterns, archeological evidence does not always support the assertion of permanent settlement with stabilized conditions. Indeed some mounds may not represent villages at all in the accepted sense. These points may be illustrated by the site of Rana Ghundhai where the lower 14 feet of deposit lack any structural remains whatsoever (Piggott 1950:121), and where the occupation is disrupted several times, with subsequent changes in the quality and nature of the occupying culture. The importance of social and political disruption in semi-arid areas cannot be overemphasized. Land that passes out of agricultural use is very difficult to reclaim. The effects of cultural disruption may be seen dramatically elsewhere in the case of North Africa (Baradez 1949), Palestine (Evenari *et al.* 1958), and South Arabia (Bowen 1958). The rapidity of change of this type is well illustrated by the history of the area around the site of Nippur in southern Iraq. When visited some 60 years ago it was reached by boat through marsh and swamp bordering canals. Unrest led to the abandonment of these canals, and today the visitor is confronted with a landscape of shifting sand dunes. With the re-establishment of tranquil conditions (as is also the case in Baluchistan) the land is again slowly being brought under cultivation (for Baluchistan see the Imperial Gazeteer 1908a:293-294).

The agricultural pattern in Baluchistan is affected strongly by four natural factors: (1) prolonged periods of drought affecting small areas; (2) floods that sweep away soil built up patiently during generations; (3) overcultivation of sailaba or khushkaba lands causing severe erosion; and (4) denudation of the hills.

A study of recent rainfall figures shows how extraordinarily capricious is the variation of annual rainfall. It is not in the least unusual for several disastrously dry years to follow one another. Nor is this all—the areal distribution is such that a limited area may so suffer while another perhaps 20 miles away is saved by just enough rain to raise a crop. Statistical analysis of the available figures for certain stations in Baluchistan shows that the average incidence of summer droughts around Quetta is about one in six years, and that of either winter or summer droughts in Makran is about the same, if not more frequent. Comparison of two adjacent stations (adjacent in this context can mean 100 miles apart) shows that, while there is a tendency for the two records to follow one another as regards years of extreme drought, the exceptions to the tendency are numerous. The effect of such variation can be easily imagined. Suppose, for example, that a community in Kolwa district of Makran returns from its summer grazing grounds on the Kalat plateau ready to cultivate its winter barley. If no rain falls in its lands it will have to move in search of a place—perhaps still in the Kolwa basin, perhaps farther away—where rain has fallen. The first time this happens it will be accepted as a normal cultivating risk. If it happens for two years in succession the people will probably start a new settlement in the new area. A disastrous flood might have much the same effect, since local rainfall intensities of more than one inch per hour for short periods are not infrequent. Such floods have washed away part of the important site of Anjira in central Kalat (De Cardi 1959:17). In such cases the group might move immediately. Over-cultivation, particularly in khushkaba farming, in which little or no silt is brought onto the land to renew its fertility, might be a more gradual but no less compelling force in the same direction. Given such natural forces in an area like Kolwa, it may be argued that the very harshness of nature could by itself account for a multiplicity of sites of which at any one time only a fraction would be occupied. That such changes do occur is indicated by the fact that when Stein passed through Bal Gattar in Makran during the spring, he saw no cultivation at all, whereas three decades later crops were being harvested from small khushkaba areas on the edge of the plain, and there was a small temporary village.

Variation in precipitation and flooding is also influenced by denudation. Although denudation in itself neither supports nor opposes the theory of desiccation, its effects are similar to those of desiccation in producing desert conditions, and the appearance of such conditions partially accounts for Stein's hypothesis. Denudation appears to be increasing in Baluchistan. A study of the flood records of the Nari River shows that only 25 years ago the hydrographs of floods were consistently flatter than they are now. Duration was greater and peak discharges lower. These phenomena are probably connected with denudation of the catchment area. Over-grazing is undoubtedly one major contributing factor for this, and one has only to observe a piece of fenced land to see the change in vegetation cover within a very short time. In some areas, particularly those near Quetta, the older inhabitants insist that in their young days the country carried many more trees and bushes. If true, the reason for the decrease probably lies in the excessive cutting of firewood for Quetta and even for Karachi. Quetta itself, with its nonmigrant population requiring winter heat, must have accounted for much denudation in the last 50 years, with a consequent increase in arid conditions.

In summary, with regard to climatic change in Baluchistan, it may be said that, although certain changes may be observed in the recent past, and while similar changes may have occurred at various times in the more remote past, nevertheless the evidence which has been brought forward to demonstrate these earlier changes is not convincing. In each case alternative inferences may be drawn which need not lead to the same conclusion. It is only when the inferences are *selected* with the desiccation hypothesis in mind that convergence occurs. The major lack of any chronological order for the structures and sites referred to, based on accurate excavation, and the consequent lack of any real knowledge of the functional nature of the cultures present (now described only as ceramic styles), makes the interpretation of archeological data in this area in regard to population size, etc., extremely unsatisfactory.

Let us now turn to the evidence relating to the ancient climate of the Indus Valley as first formulated by Sir John Marshall in the initial excavation report on Mohenjo Daro (Marshall 1931:2). Accepting Stein's thesis of desiccation in Baluchistan, Marshall extended

the implications to the Indus area, using archeological data in support of his conclusions. The latter have been uncritically repeated down to the present day (Vats 1940:4; Piggott 1950:134-135; Wheeler 1956:5-6). They may be briefly restated: (1) the size of the two major cities of Harappa and Mohenjo Daro indicates a flourishing agriculture "out of all relation to the present landscape and not wholly explicable by the possibility of elaborate former irrigation works . . ." (Wheeler 1956:6); (2) the fauna depicted on the seals lives in jungle-forest and hence indicates a wetter climate; (3) the existence of an elaborate drainage system may have been due to heavier rainfall; (4) the use of large quantities of burnt bricks at nearly all Indus sites implies plentiful fuel and, therefore, greater rainfall.

How valid are these points? Each raises a set of problems which require separate discussion. In regard to agricultural capacity, for example, it is important to consider what the Indus Valley would have been like if, with the same climate as today, it were deprived of its irrigation systems, and if the hill catchments of its headwaters were in their primitive state. These hill catchments have been much denuded in recent times by excessive extraction of timber and, furthermore, now support a comparatively large population. As there are virtually no known prehistoric settlements in this area, it is reasonable to assume that 4,000 years ago the population was sparse and the catchments covered with the kind of "jungle"[8] which now only flourishes where permitted by man.

Under such circumstances there would still have been extensive floods bringing down immense quantities of silt (the rise in plain level at Mohenjo Daro is over 25 feet above the lowest excavated occupation), but with one important difference. Today the annual flood caused by the melting of the snow in the Hindu Kush, Karakoram, and Himalayas—a fairly regular event of without great variations—has superimposed on it floods of very high intensity due to monsoon rains falling on denuded catchments. In prehistoric times the floods due to melting snow were probably much the same as today, but the monsoon floods would have had a flatter form of hydrograph due to the retardation of run-off by vegetation. There would have been, therefore, a flood regime having less variability than it has today. The flood recession would have left a new layer of fertile silt, as well as a large area of fertile soil which would have absorbed its "field capacity" of water for a depth varying with the duration of the flood, but quite sufficient for shallow-rooted crops. This absorption is the basis of the sailaba or primitive flood irrigation practiced in Baluchistan today. Such silty soil has the characteristic of holding water for a considerable period and, barring the kind of flood catastrophes that occur even nowadays, could have supported a crop sufficient to feed several Mohenjo Daros. Even 25 years ago, before much of the contemporary irrigation system was built, the Larkana district (around Mohenjo Daro) was the richest agricultural part of Sind and supported its densest population (Pithawalla 1951:43; Spate 1957:469).

Another effect of the Indus floods would have been to secure a reasonably high water table, probably for some months, for a considerable distance on either side of the river channel. This water table could have been tapped for winter cultivation. Marshall himself long ago pointed out that the richest grainlands in Sind before modern irrigation were along this strip. He also noted that "in spite of its natural advantages, there are still many patches of salt wilderness or stretches of unreclaimed jungle interrupting the cultivation" (Din 1908:12; Marshall 1931:1; Pithawalla 1951:42-43). These areas of salty or *kalar* soils are the result of waterlogging of the soil, frequently brought about by seepage from large modern canals built without accompanying drains. Subsoil moisture fed by the canals is drawn to the surface by capillary action and deposits a saline efflorescence in the course of evaporation. This process occurs especially in those areas not affected by seasonal flooding and leads to the agricultural abandonment of the areas where it occurs. Studies in Iraq have recently shown the same effects in ancient Mesopotamian agriculture (Jacobsen and Adams 1958). The plain immediately adjacent to Mohenjo Daro is at present salty and sustains only dwarf tamarisk, baboul, camelthorn, and stands of coarse *kanh* grass. Nearby, however, the countryside is covered with cultivated fields. Such use and disuse of local stretches of land certainly need not be explained solely in terms of a reduced annual rainfall.

There is then also the question of the relationship of this landscape to the fauna known from Harappa and Mohenjo Daro (Marshall 1931; Prashad 1936) in the form of seals, figurines, and animal bones. It will be seen that, when the actually known zoological habitats of these animals are reviewed, rather than the stereotyped habitat popularly associated with them, they all share a habitat range which includes a

combination of grass-jungle, marsh, and scrub forest, a description which fits well the riverine strip of the Indus. In describing the local environment of the Indus area, stress is usually laid on the sandy desert regions flanking the valley as indicating the impossibility of habitation without a changed climate; for example, "... almost the whole terrain, except for the riverine strips and artificially irrigated areas, is now sandy desert with, at most, a covering of desert scrub or small bushy trees such as the tamarisk" (Wheeler 1956:5). Since all of the known sites of this civilization lie along the present and former channels of the river the "riverine strip" is a major key to the understanding of the environment, especially in terms of fuel supplies and fauna. All along this strip in which shallow ground water is present for part of the year there is, wherever it has not been cleared for cultivation, a gallery forest of tamarisk, baboul, kindi, shisham, and other trees. Areas of dense grass and marsh also occur.

Now it is said that the archeological presence of the tiger, the rhinoceros, and the elephant indicates a wetter climate, and that the rarity of the camel and the absence of the lion argue against drier conditions. But what are the known historical habitats of these animals? The tiger, although not reported from lower Sind, is known from Khyrpoor State and Upper Sind. "From Sukkur upwards it is said to occasionally issue from its cover, which is the dense fringe of tamarisk bushes and long grass along the banks of the river ..." (Murray 1884:26) which provides its three basic needs—water, shade, and prey. The tiger lives on elephant, bison, buffalo, deer, wild pig, bear, porcupine, and domesticated animals. Most of these animals are known from the present and ancient Indus fauna. The lion, it is true, is missing from the representational art of the Harappans and has not been found among the animal bones excavated. On the other hand it appears to be illustrated on Nundara pottery (Piggott 1950:87), and was found in Sind as late as 1810 (Blandford 1888-91:57). In 1935 Admiral Philip Dumas reported seeing a lion at close range near the Bolan Pass, south of Quetta in Baluchistan (Harper 1945:294). Alexander the Great was presented with tame lions by the Malloi and Oxydrakai who occupied the area between the Chinab and Beas rivers in the Punjab. Pocock points out that, "Except that lions generally frequent more open country and are less secretive and more regardless of exposure than tigers, the habits of these two great cats are on the whole tolerably similar" (Pocock 1939:221). The camel, the bones of which were found at Mohenjo Daro (Marshall 1931:666) and Harappa (Prashad 1936:9, 58), suggests a dry climate, but in view of a complete lack of information as to its natural range of habitat, does not support the arguments either way. The rhinoceros on the other hand is known to have inhabited the banks of the Indus up to about 300 years ago as far west as Peshawar (Praten n.d.:407; Blanford 1888-91:473; Harper 1945:376). It lives by preference in areas of heavy grass-jungle and marsh along the river and rarely enters true forest or hilly areas.

It is impossible to review here all of the prehistoric Indus animals, but perhaps one or two additional ones, the bones of which have been excavated, should be added to make the point that a wet climate is by no means necessarily indicated. The hog-deer and wild bear still inhabit the "acacia-lined banks of the Indus and the thick tamarisk-fringed delta" (Murray 1884:ix). The water buffalo "haunts the densest and highest grass-jungle or reeds, but is also found at times in open plains of short grass or amongst low bushes, but rarely in tree-forest" (Blanford 1888-91:493). The mongoose is "found in hedgerows, thickets, groves of trees, cultivated fields, banks of streams, and broken bushy ground, but not commonly in dense forest" (Blanford 1888-91:124). Equally important, due to its sensitivity to moisture, is the identification of the only land snail species found at Mohenjo Daro as *Zootecus insularis* (Ehrenberg)—a native of arid regions (Marshall 1931:673). The only remaining major animal found in the fauna, the elephant, has never been reported west of the Central Provinces in India, although the possibility of a more western extension in earlier times cannot be ruled out. At the same time the extent of the Indus Civilization makes the importation of these animals from its periphery a perfectly reasonable possibility. It is clear, therefore, that the faunal evidence weighs heavily in favor of an environmental regime largely like the one now seen in the flood plain.

The conclusions drawn from the fauna as to the nature of the habitat area in antiquity find substantial support from the botanical evidence recovered from Harappa as a result of the 1946 excavations by Sir Mortimer Wheeler. The botanical study of the remains concludes, "All botanical evidences lead one to think that four thousand years ago, near about Harappa there was a scrubby forest with pockets of marshy land and tall grasses, where rainfall was limited to a few months in the year" (Chowdhury and Ghosh 1951:18). Such an environment had already been inferred from

zoological remains, especially regarding the rhinoceros, in Prashad's statement that "probably there were marshy forest areas in the neighborhood of Harappa where the rhinoceros was found" (Prashad 1936:31). Added to these data is the fact that cotton, a plant which thrives in a hot dry climate, was being grown. Taken as a unit, the available zoological and botanical evidence does not support an inference of large true forest areas based on heavier rainfall; on the contrary, it indicates an environment little changed from that of the present day.

Another major piece of evidence put forward in support of the desiccation hypothesis concerns the use of the drainage system at Mohenjo Daro for rain water. A visual inspection (as well as an examination of the published plans and sections) leaves considerable doubt as to the function of this system. For a closely built city having approximately 80 per cent of its area composed of roofs, the capacity of these drains seems quite inadequate for storm water; nor is there any evidence of the very large main storm drains into which the smaller ones might conceivably have discharged—the only large drain being considered as an outlet for the large bathing tank. Furthermore, some of the streets, like some of the houses, apparently had no drains at all. In a few cases the drains from the houses empty into sediment pits which clearly could not have absorbed storm water.

Consider a storm having a rainfall intensity of no more than one inch per hour which, for short period storms, is quite common today. A short period storm, that is, a storm lasting about 20 minutes, would almost certainly last longer than the time of concentration for any one drainage area of this small (by modern standards) town. The aggregate peak run-off from the assumed total area of Mohenjo Daro, excluding the citadel—that is about 3,500 feet by 2,200 feet—would have been, therefore, around 150 cusecs. If the excavated drains were part of a proper storm water drainage system, the main storm sewers would presumably have followed the main streets, and certainly may be assumed to have had eight to perhaps ten exits from the city. Each main sewer would have to carry from 15 to 20 cusecs and would have required a considerable diameter. At least one of the streets already excavated is exposed near the city boundary where the sewer would have attained its maximum size, but there is no indication of any such feature. In the last analysis the size of the storm drains depends, for a small area, on the intensity of individual rainstorms and not on annual rainfall. There is no evidence in areas bordering the monsoon that such intensities are dependent on annual rainfall. High annual totals rather reflect a greater number of storms. It is clear, then, that the existing drainage system, which is inadequate to carry off the storm water from an average present day short period storm, cannot be accepted as evidence indicating a heavier rainfall in antiquity.

The arguments about the use of burnt bricks also bear reconsideration. Inquiries as to what wood is used today for burning bricks in Sind elicit the reply, "tamarisk or any other wood available." That there is abundant wood available along the alluvial strip which borders the river to a distance of about 12 miles on either side is indicated by the fact that the gallery forest was providing wood for export as far back as 1908 (Imperial Gazeteer 1908b:418). It is still the source for timber used for boats and fuel, for tanning, and for livestock fodder. Large quantities of firewood may be seen today piled in yards along the railway waiting for export to the nearest market. Nevertheless, it is said that the quantity of bricks at Mohenjo Daro would have required more fuel for burning than is now available. (Animal dung, an important auxiliary fuel, is not included in fuel calculations, although it may have been used in addition to wood.) But, like Rome, the city was not built in a day. Indeed, if the estimates are correct (and this is by no means demonstrated in the studies made to date) there are seven rebuildings covering a period of perhaps a thousand years. Now one *lakh* of bricks (100,000) requires about 1,800 *maunds* of wood. This represents about 80 to 100 reasonably well-grown tamarisk trees. These bricks would be sufficient for about 7,000 cubic feet of brickwork. It is fairly obvious that over a period of a thousand years, or perhaps 50 generations of tamarisk, there would be little risk of exhausting the local gallery forest. A rough, but very liberal, calculation indicates that the fuel for each rebuilding of the city could, in fact, have been obtained from about 400 acres of gallery forest—assuming that each rebuilding was done in one short operation. With rebuildings at average intervals of about 140 years, it is obvious that if necessary the same 400 acres could have been used each time.

In addition to the burnt brick, one should note that structures of sun-dried mud brick were also used in this period. In some cases the mud brick was laid in alternate courses with burnt brick (Mackay 1943:7;

Vats 1940:12). The argument is made that, "Had the climate been as dry and the rainfall as scanty as it is today, it can hardly be doubted that they would have used sun-dried bricks (which are cheaper than burnt ones)" (Marshall 1931:2). This is a curious statement in view of the fact that (1) sun-dried mud bricks *were* used in buildings other than the central and most important structures; (2) the burnt brick buildings themselves were apparently covered with sun-dried mud plaster; and (3) the modern population builds structures of burnt brick because of the social prestige implied in the ability to pay for them. Cheap materials obviously are not necessarily the first choice of people in executing their requirements.

There was also the problem of making important buildings (including the granaries in which the crops were stored) flood-proof. There is some evidence that some of the rebuilding at Mohenjo Daro was necessitated by major floods which not only damaged the existing buildings but also engulfed them in silt, at the same time raising the level of the plain. Apart from these major floods which appear to have had an average frequency of about one every 140 years, there must have been in many, if not most, years the virtual certainty of some flooding. Burnt brick construction would have withstood this, even with mud mortar, better than mud brick. At Harappa there seems to be evidence of a burnt brick protective bund (Wheeler 1953:19); mud bricks or an unprotected earth bund would have been frequently breached.

Along with these considerations in connection with brickwork, there is the question of the type of rain. Modern Mohenjo Daro has an annual rainfall normal of about four inches, nearly all of which falls in summer. This low normal reflects a small number of storms rather than many small storms. This is a general characteristic of rainfall in arid or semi-arid areas adjacent to monsoon areas. Mud brick is used in various countries in regions having up to 20 inches of rainfall, provided that the rain occurs in heavy storms with drying periods between. Even within the monsoon belt itself, as at Chittagong in East Pakistan, poured mud or pisé walls are used at the present time, built on a raised earthen platform and protected by slightly over-hanging roof-eaves. So, even had the rainfall of the prehistoric period been five times that of today, mud brick still *could* have been used. Great caution is, therefore, indicated in drawing conclusions with regard to climatic conditions from the building materials used.

In a brief general review of a problem as complex as that of ancient climate, it is of course impossible to cover all of the evidence which might be discussed. Nevertheless, it is felt that the comments made in this discussion, while not conclusive in themselves, do throw the inferences on which the hypothesis of progressive desiccation has heretofore been based into serious doubt. In almost every case the evidence—hydrographical, zoological, botanical, archeological, and architectural—fails to substantiate the hypothesis. The available evidence simply does not demonstrate that climatic change of any major proportion has occurred. Nor does it support Marshall's statement that, while the individual facts are not conclusive, collectively they certainly point to desiccation (Marshall 1931:2).

This review emphasizes, if nothing else, the importance of integrating *all* types of evidence and of checking on the inferences drawn from them. The lack of such integration and the sparseness of the data point out the great need for additional study and field work. Studies of the relationship of archeological sites to aspects of the present environment—soil types, vegetation areas, rainfall areas, etc., must be undertaken before differences with the past can be adequately measured. Equally important, but sadly neglected as a rule, are studies of present-day cultures from the point of view of their archeological implications. Work of this nature must be paralleled by excavations at key sites aimed at placing ceramic wares in their proper cultural context and at establishment of the non-ceramic content of these cultures with a view to the interpretation of the overall cultural patterning of the area. Especially pressing is the need for a reliable chronology of the Baluchistan pottery and a detailed mapping, period by period, of the locations of inhabited sites in relation to their geographical setting. A re-examination of the major sites of Harappa and Mohenjo Daro with respect to the problems of drainage, reconstruction, etc., would be of great value, as would some information on their actual areal extent. Further information on the agricultural techniques used by these people would also be a major contribution to our understanding of these problems.

That present-day conditions reflect serious alterations in the natural environment of the Indus-Baluchistan region cannot be denied in the light of recent hydrographic evidence. The gradual extinction of fauna and the destruction of flora, coupled with a rapidly increasing population, can only result in a

changed balance in the natural forces in the midst of which man finds himself. To sort these forces out, to indicate in detail "how in an arid land human factors can . . . produce results which . . . might easily be mistaken for those of true desiccation"(Stein 1931:34), and to tell "how far that change is due to 'natural' causes and how far to sheer human improvidence" (Wheeler 1953:6) is the task before us. It is a significant one in that it deals not only with the past, but also in that it bears on the problems of the present and the future development of Pakistan. The historical perspective which archeology can provide is essential to the understanding of the limitations and potentialities of human beings and the landscape which they inhabit. It is to this end that the authors submit their effort, hopeful that it will lead to further discussion and eventual clarification of the issues involved.

NOTES

[1]This paper is the outcome of hydrographic field studies made by R.L. Raikes during the last decade as consultant to the government of Pakistan in Sind and Baluchistan, and an archaeological field study of Indus sites made by R.H. Dyson, Jr., during 1957 with a special grant from the late Mrs. Robert L. Logan of Philadelphia.

[2](1) Nihing Kaur—Kech Kaur—Kolwa—Maski—Nal—Khozdar, with offshoots to such places as Jhal Jhao; (2) Chaman (?)—Quetta—Mastung—Kalat—Khozdar; (3) Chaman(?)—Quetta, Bolan Pass—north end of the Kacchi Plain, where it stops, but with offshoots to Isplinji, etc. in the hills; (4) Khozdar—Goru—Mula River—west side of Kacchi Plain, but not spreading out onto the plain; (5) Khozdar—Kolachi River—Gaj River—lower Sind; (6) possibly a route from Zhob Valley—Loralai—Bejie—Nari—Kacchi Plain, stopping there; and probably subsidiary routes such as Kalat—Johan—Moro River, Bibi Nai—Bolan.

[3]See routes (3), (4), and (5) in note 2.

[4]Reported by R.L. Raikes to the Department of Archaeology, Government of Pakistan. Editor's Note: Judeir ko damb is now known as Judeirjo Daro.

[5]*Ibid.*

[6]*Ibid.*

[7]Also found by R.L. Raikes at Isplinji near Quetta.

[8]From Sanskrit *jangala*, desert. Originally uncultivated land, hence land overgrown with brushwood, etc Now any tangled mass of vegetation or impenetrable thicket of tropical plants.

The Indus Valley Culture

GURDIP SINGH

The Indus Valley culture, or the Harappa culture as it is called from the name of the village occupying the site of its northern capital, on the river Ravi in West Pakistan, is known to the world as the largest of its three most ancient civilizations (Wheeler: 1953). The known remains of this culture, with all its ramifications, occupy a broad triangular area in north-west India and West Pakistan, extending from the foot of the Himalayas to the Makran Coast, Gujarat, Bahawalpur and north-western Rajasthan, measuring nearly 1,000 miles from Sutkāgen Dor near the coast 300 miles west of Karachi to Rupar at the foot of Siwaliks in the extreme north-east. Further explorations have now pushed the limits of this culture from Cambay in the south to the Jumna Basin, about 30 miles north of Delhi. The Harappa culture is essentially a riverine culture as the remains of its towns and villages, with rare exceptions, occupy the banks of the present river systems or former ones now occurring as dry river beds. The culture was initially considered to have extended over much of the third millennium B.C. up to the mid-second millennium B.C., but in recent years it has been assigned a much more limited time range, 2300-1750 B.C. (Agrawal, Kusumgar and Sarna 1964b), by means of close radiocarbon datings from Kot Diji, Kalibangan and Lothal. The civilization itself is shown to have been based on a secure foundation of an agricultural-cum-urban economy (cultivating wheat, barley, cotton, field-peas, melon, sesame and dates), but the conditions leading to the immediate development of such an advanced economy have tended to remain obscure. It has often been assumed that a civilization such as that of the Indus cannot be visualized as a slow and patient growth (Wheeler 1966:61), an assumption which has been accepted generally in view of the sudden appearance of the culture in an accomplished form all over north-west India.

It is known for certain that the practice of cereal farming was in force in the "fertile crescent" in the Near East several millennia before the advent of the Indus culture, and that two great riverine civilizations had started ahead of it in Mesopotamia and in Egypt. But, strangely enough, there is no physical evidence to base any direct derivation of the Harappan culture from either of these civilizations. The arts, crafts and the characteristic Harappan technology, manifested in the most highly individualized form of script, stand out clearly from any other known culture, either preceding or following this period. While the origin of the Indus culture is still shrouded in mystery, the mode and the prime causes leading to its sudden decline are still less understood.

One of the most debated of the issues concerning the Indus people in recent years has been regarding the postulated occurrence of higher rainfall conditions during the period they are known to have flourished in north-west India. Ever since Stein (1931), on the basis of the occurrence of "gabar bands" (dam-like structures constructed artificially by prehistoric men), suggested the possibility of a significant decline in rainfall of Baluchistan since prehistoric times resulting from climatic change, other authors have produced additional evidence, in one form or another, to support the hypothesis (Marshall 1931; Vats 1940; Wheeler 1953, 1966, 1968; Chowdhury and Ghosh 1951). Deductions on the nature of climate during Harappan times have so far, however, rested on indirect evidence provided by human artifacts, such as sculptures, drawings and engravings of animals and plants on

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potsherds and steatite seals, burnt bricks, animal bones, wood and charcoal remains, flooding horizons and the occurrence of "gabar bands." In recent years, the testimony to higher rainfall argued for each of these lines of evidence has been questioned by some authors (Raikes 1964, 1965a, 1965c; Raikes and Dyson 1961; Dales 1966c). Without going into details either for or against the issue at this stage, it may suffice to say that sufficient weight had been attached to the counter-arguments for Dales (1966c:131) to say: "Convincing evidence, collected from both archaeological and natural science investigations, refutes the popular theories of appreciable climatic change in the South Asian area during the past four to five thousand years (Raikes 1965a; Raikes and Dyson 1961). Climate has thus been practically eliminated as a major factor in the environmental fortunes of the Harappan civilization."

The interests of the present author, who is primarily a plant palaeoecologist, do not rest directly in the line of archaeology. Recently, however, a series of new evidences has emerged from pollen-analytical studies of salt-lake deposits carried out by the author in an area centring on the Rajasthan desert in north-west India, which reflects closely on the nature of climate of the period of the Indus Valley culture and has largely prompted the present contribution (Singh 1970). Besides the post-glacial climatic sequence, the studies have also brought out evidence suggesting the possibility of early primitive agriculture in north-west India, antedating the Harappan culture by about five thousand years.

The present contribution is an attempt to open the question of the nature of climate governing the Indus culture once again and to consider the rise and fall of this culture in a palaeoecological context, with special reference to the possibility of a gradual evolution of the art of agriculture in north-west India since early post-glacial times.

The Area Investigated

The area covered by the present investigations is broadly comprised of the State of Rajasthan (20° 3′ N. – 30° 12′ N. latitudes and 69° 30′ E. – 78° 17′ E. longitudes), and some bordering areas in the adjoining States of the Punjab, Haryana and western Uttar Pradesh, in north-west India. The investigations comprising studies on the late-Quaternary history were confined to the Rajasthan territory, whereas the work on surface samples for determining basic criteria for the interpretation of pollen diagrams was extended to the other provinces.

(1) Physiography

The territory is broadly subdivided into two geographical units by the Aravalli Range, which runs in a north-east – south-west direction. The eastern half, which constitutes the northern part of the Central Highlands, is endowed with comparatively higher rainfall, and its south-eastern parts conform very closely to the pattern of tropical humidity found in central India. The western half, also called the Western Sandy Plains (Misra 1967), on the other hand, is a part of the Great Indian Desert, most of the western part of which is now in Pakistan. The part included in the State of Rajasthan is mostly an arid, sterile and sandy desert in the west which grades into a semi-arid steppe in the neighbourhood of the Aravalli Range. The only river of any consequence in this region is the Luni, which rises in the hills near Ajmer and flows west by south-west into the Rann of Kutch. The Ghaggar (Saraswati of Vedic times) once flowed through the northern part of Rajasthan and is said to have joined the river Indus, but now it is dry except during the rainy season, when its water loses itself in sand a mile or two west of the town of Sadulgarh.

(2) Climate

The area is characterized by a progressive fall in the mean annual rainfall towards its core, centring on the Thar desert, with less than 10 cm. average annual rainfall. One witnesses the occurrence of more or less eccentrically placed zones of increasing rainfall round the central core which, proceeding outwards, are here described as Arid (10-25 cm. average annual rainfall), Semi-Arid (25-50 cm.), Semi-Humid (50-60 cm.) and Humid (60-100 cm.). The climate of north-west India as a whole is divisible into four seasons: winter (cool and dry), spring or pre-monsoon (hot and dry), summer or monsoon (hot and wet) and autumn or post-monsoon (warm and dry). Geographically, the area is situated in the normal latitudes of sub-tropical anticyclones in winter and the deserts associated with them. In general terms, the aridity in the core area may be ascribed to the rather sharp north-westward boundary to the moist current of the monsoon, as the area lies a little too far west of the main monsoonal upper air flow to receive such monsoonal rainfall, save in exceptional years. The air

circulation in winter is marked by subsiding, low-level anticyclonic flow (Raman and Dixit 1964; Frost and Stephanson 1964). On the far north-west, however, the winds are of continental origin and come from the north-west. The winter rainfall resulting from western disturbances is mainly confined to the area north and north-west of Delhi and tends to decrease progressively towards the Thar desert. The Aravalli Range acts as a great divide between the western and eastern climatic provinces of north-west India. The province to the west of the Aravalis is characterized by extremes of temperature, severe drought, high velocity winds and low relative humidity. The region east and south of the Aravallis shows considerable variation in the amount of rainfall and temperature distribution even though, as a whole, the area is denoted by strong periodical rains and more or less uniform temperatures.

(3) Vegetation

It is beyond the scope of this paper to give a detailed background of the vegetation of the area but it is necessary to point out some of the critical aspects of the distribution of plant life which have allowed the use of the past distribution of certain individualized plant species as climatic indicators for the present investigations.

As is true of most other natural phenomena, the vegetation of the area is greatly influenced by the placement of the Aravalli Range, which divides the plant life into two unequal parts: a smaller one (generally mesophytic) east of the Aravallis, and the larger (mostly xerophytic) to their west (King 1879). The vegetation of the dividing range itself can be considered to resemble much more the eastern than the western tract. In general terms, three distinct elements can be distinguished: a Western (Perso-Arabian) west of the Aravallis, an Eastern (Indo-Malayan) east of them, and a more general element (including Indian species also) in the Aravallis themselves (Biswas and Rao 1953). The line dividing the two major phytogeographical units, the Perso-Arabian and the Indo-Malayan, passes along the Aravalli Range from the gulf of Cambay in the south-west to Delhi in the north-east (Drude 1890). In addition to the placement of the mountain barrier, this line is also determined approximately by the 50 cm. isohyet line within the State of Rajasthan. In view of the critical nature of the above boundary, any recognizable shift observed in the past vegetation, *vis-a-vis* the two floras, becomes diagnostic of a change of climate in the distant past. The selection of the sites investigated is clearly motivated by the above considerations.

(4) Sites Examined

Pollen-analytical studies were carried out on three salt lakes in western Rajasthan, at Sambhar, Didwana and Lunkaransar, and one freshwater lake in eastern Rajasthan, at Pushkar in the Aravallis. Both the lakes at Sambhar (27° N. 75° E.) and Didwana (27° 20′ N. 74° 35′ E.) lie in the Semi-Arid belt (25-30 cm. average annual rainfall), while the lakes at Lunkaransar (28° 30′ N. 73° 45′ E.) and Pushkar (26° 29′ N. 74° 33′ E.) are respectively in the Arid (less than 25 cm. rainfall) and the Semi-Humid (50-60 cm. rainfall) belts. Of the four lake-site investigations the evidence from Pushkar, which refers mainly to the post-Harappan period, is not considered in the present paper. Soil samples from pre-Harappan levels at Kalibangan, the only Indus Valley site excavated in Rajasthan, were examined for pollen-analysis. A brief summary of the palaeoecological results from one of the salt lakes, namely Sambhar, has already been published (Singh 1967).

Stratigraphical, Palaeoecological, and Cultural Considerations

The known time range of the Indus culture, which is about five hundred years, is only a small fraction of the Holocene period. While it would be quite futile to dwell in detail on the findings pertaining to the entire post-glacial period, it is necessary to have a long-range perspective of the climatic sequence for a consideration of the development of the late-Holocene cultures, including the Harappan culture, in their natural settings. It is therefore proposed to present an outline of post-glacial environmental history preceding the Indus Valley culture, together with some of the basic evidence, and to discuss the same in the light of the known cultural history (Figs. 29.1 and 29.2).

The environmental sequence which is built up here from the vegetational history deduced from pollen analysis has been grouped into six phases, of which all but phase I belong to the Holocene period. Phase I, which is primarily inferred from the general stratigraphy of the salt-lake basins, is pre-Holocene. Phases III, IV and VI are further subdivided into subphases IIIa, IIIb, IVa, IVb, IVc, VIa and VIb, following the zonation of the pollen diagrams. The pollen record

VEGETATIONAL HISTORY

YEARS B.P.	YEARS A.D./B.C.	POLLEN ZONES	CLIMATIC PHASES	SAMBHAR SALT-LAKE (Semi-arid belt)	DIDWANA SALT-LAKE (Semi-arid belt)
1000	A.D. 1000	E	VI	POLLEN NOT PRESERVED	POLLEN NOT PRESERVED
		BASED ON EVIDENCE FROM PUSHKAR NOT DISCUSSED IN THIS PAPER		LAKE CONTINUES TO REMAIN SALINE	LAKE REMAINS SALINE
2000	B.C.	D	V	LAKE STARTS DRYING	LAKE STARTS DRYING
3000	1000	C_3	IV c	CYPERACEAE RISES BUT MESOPHYTIC AS WELL AS AQUATIC SPECIES FAIL TO RETURN. *CAPPARIS* AND *EPHEDRA* RISE. CEREALIA TYPE POLLEN ABSENT. LAKE CONTINUES TO REMAIN SALINE.	SUBDIVISIONS OF ZONE C (PHASE IV) NOT DEFINED IN THE POLLEN SEQUENCE.
		C_2	IV b	FALL IN CYPERACEAE. MESOPHYTIC AND AQUATIC PLANT SPECIES DISAPPEAR. *ARTEMISIA* LOW. HALOPHYTES HIGH. FALL IN TREE RATIOS. CEREALIA TYPE FREQUENCIES RISE AND THEN DISAPPEAR. LAKE TURNS SALINE.	
4000	2000	C_1	IV a	MAXIMUM RISE IN CYPERACEAE, *MIMOSA RUBICAULIS* AND *SYZIGIUM CUMINII*. TREE AND SHRUB FREQUENCIES RISE FOR THE FIRST TIME. CHENOPODIACEAE/AMARANTHACEAE AND GRAMINEAE LOW. *CAPPARIS, PROSOPIS CINERARIA* AND *ACACIA*, WELL REPRESENTED. *ARTEMISIA, TYPHA ANGUSTATA* AND *OLDENLANDIA* CONTINUE. *MAYTENUS* AND *EPHEDRA* LOW. FRESHWATER CONDITIONS CONTINUE. CEREALIA TYPE POLLEN FREQUENCIES RISE.	MAXIMUM RISE IN CYPERACEAE. TREE AND SHRUB FREQUENCIES RISE FOR THE FIRST TIME. *SYZIGIUM CUMINII, MIMOSA RUBICAULIS, TYPHA ANGUSTATA, OLDENLANDIA*, AND *ARTEMISIA* REPRESENTED. GRAMINEAE LOW. *CAPPARIS, EPHEDRA, PROSOPIS CINERARIA* AND *CALLIGONUM* RISE. FRESHWATER CONDITIONS CONTINUE UP TO THE MIDDLE OF ZONE C. CEREALIA TYPE POLLEN CONTINUES TO BE SEEN.
5000	3000				
6000, 7000, 8000, 9000	4000, 5000, 6000, 7000	B	III	RISE IN CHENOPODIACEAE/AMARANTHACEAE, *MAYTENUS, CAPPARIS* AND *EPHEDRA*. CYPERACEAE AND GRAMINEAE FALL. *ARTEMISIA, MIMOSA RUBICAULIS, OLDENLANDIA* AND *TYPHA ANGUSTATA* VERY WELL REPPESENTED. FRESHWATER CONDITIONS CONTINUE. CEREALIA TYPE POLLEN SEEN, FIRST IRREGULARLY, THEN MORE FREQUENTLY.	CHENOPODIACEAE/AMARANTHACEAE, *MAYTENUS, CAPPARIS* AND *EPHEDRA* WELL REPRESENTED. CYPERACEAE LOW. GRAMINEAE, *ARTEMISIA, TYPHA ANGUSTATA* AND *OLDENLANDIA* HIGH. FRESHWATER CONDITIONS PREVALENT INSIDE THE LAKE. CEREALIA TYPE POLLEN SEEN, FIRST IRREGULARLY, THEN MORE FREQUENTLY.
10.000	8000	A	II	LACUSTRINE PERIOD BEGINS. CYPERACEAE, GRAMINEAE, *ARTEMISIA, MIMOSA RUBICAULIS, OLDENLANDIA* AND *TYPHA ANGUSTATA* HIGH. HALOPHYTES AND XEROPHYTES LOW. LAKE FRESHWATER.	ZONE NOT REPRESENTED (PROBABLY ERODED AT THE POINT OF SAMPLING)
		PRE-HOLOCENE	I	NON-LACUSTRINE PERIOD, SAND-DUNES ACTIVE, POLLEN NOT PRESERVED.	NON-LACUSTRINE PERIOD, SAND-DUNES ACTIVE, POLLEN NOT PRESERVED.

FIGURE 29.1. *Schematic chart showing post-glacial vegetational, climatic, and early agricultural history in the Rajasthan desert in relation to the known history of the Indus Valley Culture in northwest India*

comes wholly from the Holocene, as fossil pollen is not preserved in the pre-Holocene sandy material. The post-glacial pollen sequences from the sites investigated are divisible into five zones termed A, B, C, D and E, of which zones B, C and E are subdivided into subzones B_1, B_2, C_1, C_2, C_3, E_1 and E_2. The original pollen diagrams, together with their detailed descriptions, will be published elsewhere. In the present discussion only phases I, II, III and a part of phase IV, which are relevant to the Indus Valley culture, are considered.

Phase I: Before 8000 B.C.

The evidence for phase I comes mainly from the

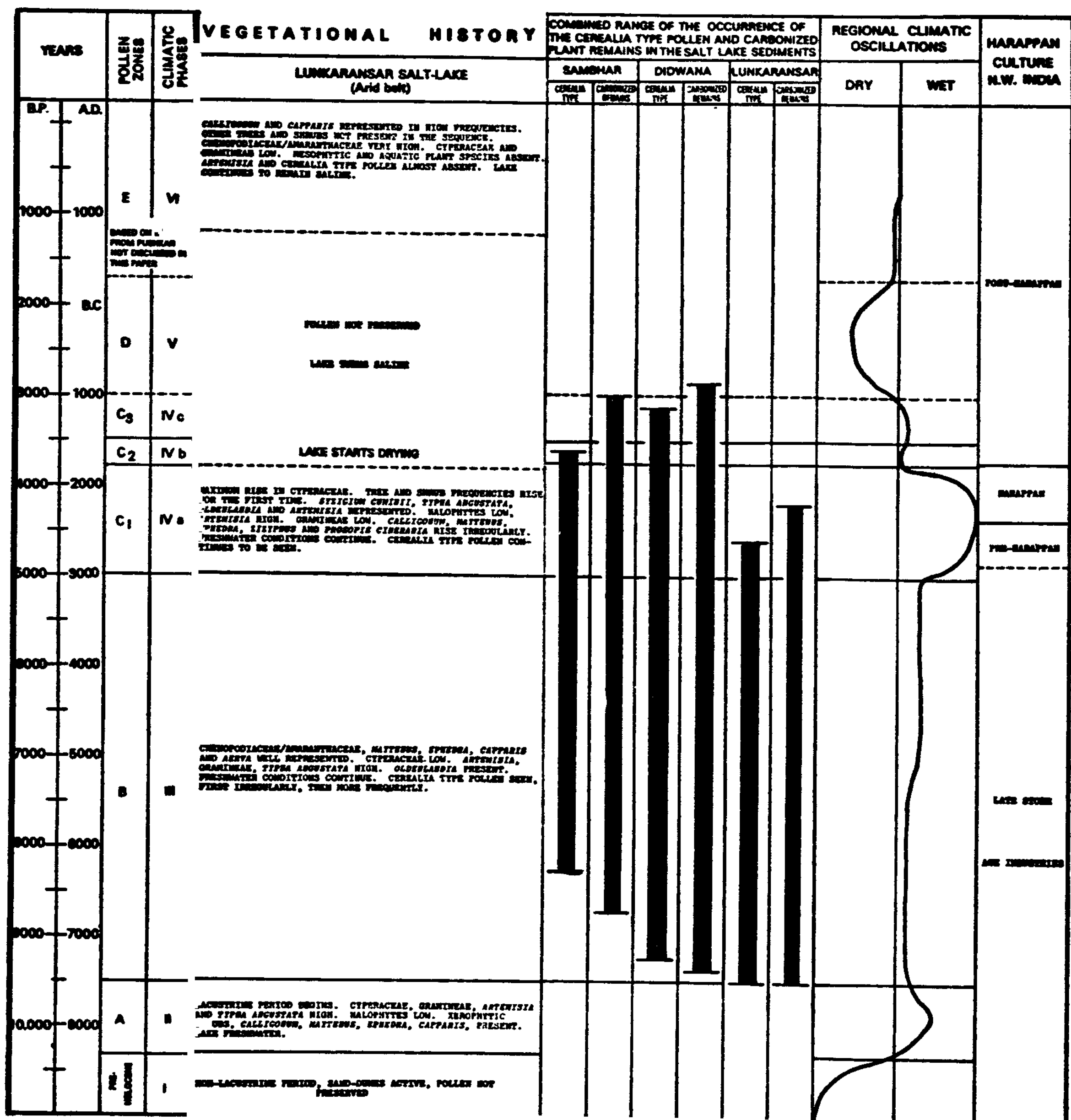


FIGURE 29.2. *Schematic chart showing post-glacial vegetational, climatic, and early agricultural history in the Rajasthan desert in relation to the known history of the Indus Valley Culture in northwest India.*

stratigraphy of the salt-lake basins. The earliest lake sediments, which date from early Holocene times at all the sites, are underlain by a thick bed of loosely packed, wind-borne sand. In each case the sand bed is seen to extend horizontally to meet the sand dunes (which are now stabilized) encircling the individual salt-lake basins. This suggests that the basins were probably formed during pre-Holocene times by the damming of ancient valleys by wind-borne sand, at a time when the sand dunes were still active. While the beginning of this phase of severe, pre-Holocene aridity is uncertain, there is evidence that the phase ended with an increase in rainfall at about the beginning of the Holocene period. This is shown by the radiocarbon assays obtained separately from Sambhar (9250 ± 115 B.P., TF-887) and Lunkaransar (9260 ± 115 B.P., WIS-405), from levels about 30-40 cm. above the sand/clay boundaries at the two sites.

From these results a date for the infilling of the lake basins can be extrapolated to around 10,000 B.P.

There is practically nothing known of the human cultures occupying north-west India during this phase. At present there appears to be a somewhat abrupt break between the largely Upper Pleistocene Middle Stone Age cultures and the exclusively post-glacial microlithic Late Stone Age cultures in north-west India. This is quite unlike the situation prevailing in southern India, where the change from Middle to Late Stone Age, that is to say from the flake to the microlithic tradition, appears to have been a process of continuous development rather than of sudden change (Allchin and Allchin 1968:78). Whether the hiatus between the Middle Stone Age and the Late Stone Age cultures in north-west India was in some manner influenced by the severe aridity prevailing during pre-Holocene times is not certain. From the present evidence it would, however, appear that the central core of north-west India was affected by severe aridity and that the territory would have been unsuitable for habitation for a long time during at least the later part of the last glacial.

Phase II: Pollen zone A: c. 8000 B.C. – *c. 7500* B.C.

This phase starts with the first sedimentation of freshwater lacustrine deposits in the lake basins, a development that appears to have occurred at about the beginning of the Holocene period around 10,000 B.P. The lake sediments consist of laminated clays and contain a fair amount of fossil pollen at all the sites, thus allowing the reconstruction of the vegetational history of the post-glacial period.

The vegetation as deduced from the pollen record in phase II, represented by pollen zone A, is comprised of high values for sedges and grasses and low values for halophytes denoted by Chenopodiaceae/Amaranthaceae. *Artemisia*, which now grows abundantly under a higher rainfall régime (above 50 cm. average annual rainfall) mostly in the Himalayan foothill plains and which is at present rarely seen in the Rajasthan desert, appears to have flourished in large numbers during this phase in both the contemporary Arid and the Semi-Arid belts. Mesophytic plants such as *Mimosa rubicaulis* and *Oldenlandia*, which now grow mostly east of the Aravalli Range and upcountry in Punjab and Haryana, are indicated as having occupied the Semi-Arid belt in western Rajasthan. There is little desert vegetation represented in the Semi-Arid belt in this phase: small quantities of *Ephedra* pollen, which is notorious for being wind-transported over long distances (Maher 1964), is, however, seen regularly; a few grains of *Maytenus*, a desert plant species, make their appearance in the later part of the sequence in this phase. As there is no further intercalation of sand layers in the lake sediments, it is suggested that the sand dunes had started to stabilize. That the rainfall was considerably more than that of the present day, with fireshwater conditions prevailing in the lake basins, is apparently testified by the presence, in both the Arid and the Semi-Arid belts, of *Typha angustata*, a freshwater aquatic species no longer seen in the Rajasthan desert beyond the 50 cm. isohyetal line. The present evidence would thus indicate that there was an excess of at least 25 cm. (10 in.) of precipitation over the present in the Arid belt in phase II.

Phase II corresponds in time to the pre-Boreal period in Europe, when temperatures are known to have started rising throughout the world, including north-west India, following the final recession of the last glacial (von Post 1946; Godwin 1956; Iversen 1954; Singh 1963). In the Rajasthan desert, as is seen above, the climatic change is suggested to have taken the form of an increase in rainfall.

The state of human cultures occupying the Rajasthan desert during this phase is largely a matter of conjecture, as work on Late Stone Age microlithic cultures in India is still in its infancy. Nevertheless, the climatic conditions of the Rajasthan area, as inferred above, appear to have been favourable enough to support freshwater bodies and perennial river courses, which in turn can be expected to have supported animal and human populations.

Phase III: Pollen zone B: c. 7500 B.C.–*c. 3000* B.C.

The initial rise in precipitation in phase II appears to have given way to slight lowering of rainfall around 7500 B.C., which was, however, not severe enough substantially to alter the overall ecological patterns already established during the earlier phase. While the sedges decline, the halophytic vegetation, represented by Chenopodiaceae/Amaranthaceae, starts a gradual rise. Desert shrubs, such as *Maytenus* and *Capparis*, begin to occur more regularly in the Semi-Arid belt. The freshwater aquatic vegetation, consisting of *Typha angustata*, *T. latifolia* and *Potamogeton*, was apparently not affected, suggesting the continuation of freshwater environments in the lake basins. Mesophytic plant species, *Artemisia*, *Oldenlandia* and

Mimosa rubicaulis, are also suggested to have continued flourishing in the Semi-Arid belt, and all except *Mimosa rubicaulis* occurred, to a lesser degree, in the Arid belt.

Phase III, which is represented by pollen zone B and ranges between *c.* 7500 B.C. and *c.* 3000 B.C., can be broadly equated chronologically with the combined Boreal and Atlantic periods of Europe (Godwin 1960). Its lower limit is determined by two radiocarbon analyses mentioned earlier from Sambhar (9250 ± 130 B.P., TF-887) and Lunkaransar (9260 ± 115 B.P., WIS-405), dating the boundary between pollen zones A and B to about 7500 B.C. at both sites.

Early Agriculture. The late Stone Age microlithic culture (or cultures) which preceded the Neolithic-Chalcolithic cultures in north-west India and is most likely to have been dominant in the Rajasthan area, unfortunately remains undated for most of the part. At a few places where the remains of this culture have been found in stratigraphical context, it tends to overlap the known Neolithic-Chalcolithic cultures in its upper levels, whereas its early history remains almost unfathomed. The earliest radiocarbon dated horizons go back to approximately 5500 B.C., in the rock shelters excavated at Adamgarh hill in the Narbada valley (Allchin and Allchin 1968). These sites, together with another microlithic site at Langhnaj, in Gujarat, show evidence of contemporaneity for a considerable period with Neolithic or later settlements in adjacent regions (Allchin and Allchin 1968). Speaking of Langhnaj, Joshi (1963:3) remarks that "on the basis of the site, it may be said that this phase is characterized by communities which have a mixed economy and might be practising some sort of primitive agriculture along with hunting and fishing, domestication of animals to some extent, burying their dead and having a slightly more settled pattern of life than the wandering folks."

In the salt-lake profiles from western Rajasthan one witnesses an extraordinary rise in carbonized vegetable remains (mostly wood fragments) in the sediments at about the beginning of phase III (pollen zone B) at all the sites. The phenomenon cannot be explained in terms of natural causes as the enhanced rate of the occurrence is maintained in layer after layer of the lake sediment, starting with phase III. It can be strongly argued that the increase in the burnt remains resulted from the introduction of the practice of scrub burning at the hands of early man, for this alone can explain the synchronous rise in their occurrence in the lake profiles of sites separated by hundreds of kilometres. It is seen that the practice of scrub burning as attested by the above evidence remained at a high level in phases III and IV (pollen zones B and C) and then dwindled away in phases V and VI (pollen zones D and E). It is of considerable interest to note that Cerealia-type pollen (grass pollen more than 40μ; size range $40\text{-}50\mu$) also starts occurring in the lake sediments early in phase III (pollen zone B) and continues to appear intermittently throughout phases III and IV at all the sites in close harmony with the enhanced occurrence of carbonized remains. The evidence of scrub burning, together with the first occurrence of Cerealia-type pollen in the lake profiles, is dated at 9260 ± 115 B.P. (WIS-405) at Lunkaransar in the extreme west and 8300 ± 135 B.P. (TF-738) at Sambhar in the extreme east. The close correlation between the occurrence of Cerealia-type pollen on one hand and the evidence of scrub burning on the other raises the obvious question as to whether some sort of primitive cereal agriculture was introduced into the area as far back as 7500 B.C.

Before proceeding any further one may, however, ask whether the Cerealia-type of pollen seen in the salt-lake profiles from Rajasthan does in fact represent cereal pollen. In this regard studies of present-day pollen spectra from 114 samples coming from 64 different sites spread all over north-west India have demonstrated that cereal pollen, mainly because of its large size, is not carried a great distance from its mother source, so that the pollen type is not seen in areas which are free of cereal cultivation. Some wild grasses can indeed be expected to contribute large-sized pollen similar to the Cerealia-type, but the introduction of such wild grasses through purely natural means over an axis of 200 kilometres (separating Lunkaransar from Sambhar) during the same time interval at the beginning of phase III and in conjunction with the first introduction of scrub burning in the territory, seems rather far-fetched. On the other hand, it would appear from the evidence that man had already started interfering with the natural vegetation in western Rajasthan at the beginning of phase III and it is not unreasonable to believe, therefore, that the burning of scrub was probably involved in the practice of some form of primitive cereal agriculture, the like of which was still prevalent with several tribal communities until quite recently in Rajasthan. It goes without saying, however, that the present evidence suggesting an exceptionally early start for cereal agriculture in north-west India needs to be supported

with material evidence from excavations at the numerous microlithic settlements found scattered all over the Rajasthan desert, and outside.

Phase IV: Pollen zone C: c. 3000 B.C.—*c. 1000* B.C.

At the beginning of this phase, about 3000 B.C., the climate seems to have taken a sudden change to wetter conditions. The period of maximum wetness, however, appears to have lasted only up to about 1800 B.C. Thereafter, the climate shows a small-scale oscillation to drier conditions between *c.* 1800 B.C. and *c.* 1500 B.C., followed by a slight reversal to a relatively weak wetter interval, lasting up to about 1000 B.C. This period of change is considered as phase IV, which is subdivided into subphases IVa, IVb, and IVc, denoting the above threefold oscillation of climate during the mid-post-glacial. Of the three subphases, only subphases IVa and IVb are relevant to the present discussion.

(*i*) Subphase IVa: Pollen subzone C_1: *c.* 3000 B.C.–*c.* 1800 B.C. The beginning of subphase IVa, which is marked by the boundary between pollen zones B and C, is dated around 3000 B.C. from two radiocarbon analyses, 5060 ± 70 (WIS-387) and 5420 ± 70 (WIS-386), from above and below the zonal boundary respectively at Lunkaransar. At Sambhar the date for the same boundary is extrapolated to 3000 B.C. from two radiocarbon determinations, 4665 ± 115 and 4510 ± 110 (TF-739 and TF-883), from about the middle of subphase IVa (subzone C_1).

Palaeoecologically, the subphase starts with a sudden rise in the frequencies of sedges and those of tree and shrub vegetation. The latter rise for the first time in the post-glacial sequence, at all the sites. The trees and shrubs mainly consist of *Syzigium cuminii, Mimosa rubicaulis, Acacia* sp., *Prosopis cineraria, Capparis* sp. and *Tamarix* sp. in the Semi-Arid belt, and of *Calligonum polygonoides, Zizyphus* sp., *Prosopis cineraria, Maytenus* sp. and *Syzigium cuminii* in the Arid belt. The sedges reach their maximum frequencies of more than 500 per cent of all other land-plant pollen in the Semi-Arid belt and are almost as prominently represented in the Arid belt. The present evidence would indicate that *Syzigium cuminii*, a mesophytic tree now growing naturally in India in areas having in excess of 85 cm. (35 in.) average annual rainfall (Troup 1921), enjoyed a luxuriant growth in the Semi-Arid belt and that it even penetrated the western extreme of Rajasthan, as far as Lunkaransar in the Arid belt. Similarly, *Mimosa rubicaulis*, a mesophytic shrub mentioned earlier in phases II and III, rises to its highest frequencies in the Semi-Arid belt in subphase IVa. *Typha angustata* continues to grow in both the Arid and the Semi-Arid belts as before. The above palaeoecological picture of subphase IVa goes to suggest that relative precipitation over the Rajasthan desert had risen considerably and that the annual average rainfall may have been in excess by at least 50 cm. (20 in.) of the present-day rainfall in the Arid belt. In the light of all this evidence the doubts expressed earlier with respect to a relatively higher rainfall in north-west India during Harappan times (Raikes 1964, 1965a, 1965c; Raikes and Dyson 1961; Dales 1966c, can perhaps now be set aside.

Pollen of Cerealia type, of the same size range as seen in the earlier phase, continues to be met with at all the sites in subphase IVa, without any recognizable hiatus. At the same time evidence for scrub burning in the form of carbonized remains also continues into this subphase.

(*ii*) Expansion of Neolithic-Calcolithic Cultures. It is of profound interest to note that subphase IVa, in terms of chronology, sees the rise of the pre-Harappan and, later on, of the Harappan culture throughout north-west India. It is generally agreed that in the plains of Sind, the Punjab and northern Rajasthan, around the end of the fourth or the beginning of the third millennium B.C., the major expansion of the Neolithic-Chalcolithic way of life took place (Allchin and Allchin 1968:112). The earliest radiocarbon evidence so far available is the date of pre-Harappan settlement at Kot Diji, placed at 2605 ± 145 B.C. The testimony of Cerealia-type pollen for the existence of cereal agriculture in the latter half of subphase IVa cannot be doubted as the Harappan people are definitely known to have practised cereal cultivation. The question is, however, relevant when the pollen is encountered in earlier horizons.

Here it may be of interest to point out that Cerealia-type pollen of the same size range is encountered in good numbers (58 per cent of total land-plant pollen) in the pre-Harappan levels of the Indus Valley site at Kalibangan, excavated by the Archaeological Survey of India. This, together with the unbroken record of Cerealia-type pollen in the pollen profiles and the evidence of forest burning from three different sites, would lead one to believe that the practice of cereal cultivation perhaps does not start with the Indus Valley culture after all but that the practice had existed in the region for a long time, indeed, as has been suggested, from the beginning of phase III. It can in

fact be argued that the significant increase in rainfall at the beginning of the third millennium B.C., attested by the palaeoecological evidence, played an important part in the sudden expansion of the Neolithic-Chalcolithic cultures in north-west India, ultimately leading to the prosperity of the Indus culture.

(*iii*) Subphase IVb: Pollen subzone C_2: *c.* 1800 B.C.—*c.* 1500 B.C. The end of subphase IVa and the beginning of subphase IVb can be dated around 1800 B.C. by extrapolation from two radiocarbon determinations mentioned earlier (TF-739 and TF-883), from subphase IVa at Sambhar. For subphase IVb the palaeoecological evidence points to a short dry period. The sedges undergo a sudden decline. All the mesophytic plant species found in subphase IVa, such as *Syzigium cuminii, Mimosa rubicaulis* and the freshwater aquatic species, disappear for good from the pollen sequence. At Lunkaransar, in the Arid belt, the horizontal stratification of laminated clays breaks down and pollen is no longer preserved in the sediment, both factors indicating that the lake had started drying out. In the Semi-Arid belt, with the disappearance of freshwater aquatic vegetation, it would appear that the lakes had started turning saline. Indeed there is some rise in the frequencies of halophytic plant species at Sambhar. All this goes to indicate that a dry period of some intensity had set in about 1800 B.C. It is a rare coincidence that the Harappan culture is known to have started declining around 1750 B.C. (Agrawal, Kusumgar and Sarna 1964b). The archaeological evidence has been interpreted to suggest that the Harappan culture met with a sudden end. The same evidence, however, also indicates that a general decline had already set in much before the final blow (Wheeler 1966:75; 1968:127), which some believe to have been struck by the Aryan invaders (Wheeler 1966:75, 1968:127; Allchin and Allchin 1968:144-56) The present evidence would suggest that the onset of aridity in the region around 1800 B.C. probably resulted in the weakening of the Harappan culture in the arid and semi-arid parts of north-west India but that the peripheral areas of the culture, such as in Gujarat and the Himalayan foothills, were not affected to the same degree. The extinction of the Indus culture may have thus been initiated through gradual decline as a result of climatic change, but the process may yet have been completed by successive invasions from the north-west by the Aryans.

Monsoon over the Indus Valley During the Harappan Period

C. RAMASWAMY

There is adequate archaeological evidence (Wheeler 1956) that the Harappans, who flourished in the Indus Valley between 2500 and 1700 ± 100 B.C., lived in very much moister climatic conditions than those which exist today. Archaeologists (Wheeler 1966; Kosambi 1965a) have also emphasized the occurrence of intermittent floods in the Indus valley during the Harappan period. These conclusions, which are based on archaeological evidence, are also supported by the following meteorological considerations.

The patterns of 1,000-500 mbar thicknesses recently published by Lamb, Lewis and Woodroffe (1966) clearly indicate that in 2000 and 500 B.C., there was a mean trough in the upper westerlies with its axis along 70° E. and extending from high latitudes to 45° N. in the month of July. These thickness charts do not indicate the existence of such a mean trough anywhere between 0° E and 160° E. in modern times.

I have made a detailed study of the daily northern hemisphere charts for 1962-66, with good coverage of data to the north of the Indo-Pakistan sub-continent, and have found that quasi-stationary troughs in the westerlies at the 500, 300 and 200 mbar levels occasionally extend into north-west Pakistan and induce monsoon activity ahead of such troughs.[1] They are deep enough to alter the configuration (Ananthakrishnan and Bhatia 1958) of the sub-tropical high to their east and are thus indirectly responsible for causing any monsoon depressions moving in a west-north-westerly direction towards longitude 78° E., to curve to the north or north-east towards the Punjab (Pakistan), Punjab (India) and Kashmir. Before the depression curves to the north, the area of heavy rainfall lies in the left front quadrant[2] of the depression. After the depression has curved to the north, however, this area of heavy rainfall shifts to the northern sector of the depression, with the active monsoon area chiefly determined by the area of high level divergence ahead of the trough in the upper westerlies.[1, 3] In the later stages of its north-eastward movement, the monsoon depression causes floods in the upper reaches of the Himalayan rivers to the west of 78° E. An example of this can be seen in the monsoon depression of September 2-9, 1966. Such depressions curving to the north or north-east were, however, only very occasional during 1891-1960.[4]

It follows that during the south-west monsoon seasons around 2000-500 B.C. deep troughs in the upper westerlies must have extended into west Pakistan far more frequently than they do now, inducing monsoon activity and also causing any monsoon depressions from the Bay of Bengal moving towards longitude 78° E., to curve to the north or north-east. These developments would have caused frequent active monsoon conditions over the entire Indus Valley, which is basically a fertile region (Stamp 1959). Thick vegetation and marsh jungles inhabited by fauna as described by archaeologists, and intermittent floods in the Indus and its tributaries, would therefore have characterized the period of the Harappan civilization.

These conclusions are further supported by the recent discovery of considerable reserves of ground water[5] in the arid region of extreme west Rajasthan close to the Indus Valley. Carbon-14 tests carried out[6] by staff of the Tata Institute of Fundamental Research in Bombay, at a place called Palana, 14 miles south of Bikaner (28° 00′ N., 73° 18′ E.) indicate that the ground water there is about 5,000-year old, this being the upper limit of the true age of the water.

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NOTES

[1]C. Ramaswamy, *Indian Journal of Meteorology and Geophysics*, Vol. 16, No. 2, 1965, p. 177.

[2]P.R. Pisharoty and G.C. Ansarı, *Indian Journal of Meteorology and Geophysics*, Vol. 8, 1957, p. 15.

[3]C. Ramaswamy, *Tellus*, Vol. 8, 1956, p. 26.

[4]*Tracks of Storms and Depressions in the Bay of Bengal and the Arabian Sea*, Indian Meteorological Department, 1964.

[5]K.L. Rao, *Indian Geohydrology*, Vol. 1, 1965, p. 2.

[6]*Report to the Indian National Committee for International Hydrological Decade*, Tata Institute of Fundamental Research, 1966.

Early Domesticated Animals in India and Pakistan

F.R. ALLCHIN

Considering that the Archaeological Survey of India recently celebrated its centenary, and that in the past two decades there has been such a spate of excavations, the available archaeological evidence bearing on the domestication of animals in the subcontinent is regrettably sparse (see map—Fig. 31.1). Before 1947 scarcely any excavation reports thought fit to mention animal remains. The work of Sewell and Guha for Mohenjo-daro (1931), and Prasad for Harappa (1936) were happy exceptions. Since 1950 a growing number of reports has appeared in which at least some reference to animal remains is made. The principal published data relate to the excavations at Hastinapur, 1950-2 (Lal 1955); Piklihal, 1952 (F.R. Allchin 1961); Maheshwar, 1952-3 (Sankalia, Subbarao and Deo 1958); Rangpur, 1953-6 (Rao 1963a); Maski, 1954 (Thapar 1957); Mundigak, 1951-8 (Casal 1961c); Nevasa, 1954-6 (Sankalia *et al.* 1960); Utnur, 1957 (F.R. Allchin 1963b); and Langhnaj, 1944-63 (Clutton-Brock 1965). There is further a considerable volume of hitherto unpublished material relating to other excavations. In some instances the scale of the excavations, or the quantity of animal remains discovered, were such that their potential usefulness is limited. Further, some of the reports provide little more than bare identifications of species, and the number giving attention to other matters, such as the changes brought about by domestication, is still very small. One of the upshots of this note is to point to the need for national centers where animal remains can be preserved and made available for study. One fears that in the past the field archaeologist often did not trouble to collect animal remains; but cases have certainly occurred in which he did so, only to find that they were later disposed of by the custodian in whose care they were placed. Finally it may well be that small collections have been retained, but their whereabouts have ceased to be generally known. My aim in this note is to draw attention to the potential interest of this subject, and to suggest some of the problems which are ripe for consideration.

The first problem must be to discover what animals were utilized at different periods and what was the history of each species. A second problem would seem to involve the origin of each domestic species, and whether it was introduced from outside in an already domesticated condition, or was locally domesticated from indigenous wild species. A third problem would be to find out what changes, if any, were consequent upon domestication. And a fourth general problem would concern the utilization of different species, for food, traction, etc., at various periods.

For the general history of domestic species in India the data available are still so slight that there is a serious danger of misinterpreting them. It is known that the final stage of the stone age, now generally referred to as the late stone age, persisted in certain refuge areas until very recent times. Proof has however been lacking that its origin was in absolute time anterior to the arrival of the first food-producing communities even if this has been generally accepted as a working hypothesis. Recently a number of excavations of late stone age caves or rock shelters have been made, and one, at Adamgarh in Hoshangabad district, has produced a radiocarbon date suggestively earlier than those of any neolithic sites so far known in India and Pakistan.[1] Peculiar interest now centers upon this and other such sites, because among the

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Mohenjo-daro had been dyed with some substance, probably a madder. The European madder (*Rubia tinctorum*) is still cultivated in Sindh (Watt 1908: 926-7).

SOUTHERN OR PENINSULAR PROVINCE

This province is comparatively more varied in climate, rainfall in particular varying between tropical rain and semi-desert precipitation. The evidence is from recent excavations and much is as yet incompletely published.

1. *Cereals*. Early evidence of wheat cultivation is restricted to the north-west of the province. Both *Triticum vulgare* and *T. compactum* have been identified by Dr. Vishnu Mittre (1961:15-7) throughout the chalcolithic occupation at Navdatoli (*c*. 1800-1200 B.C.); and its cultivation seems to have spread towards the south to occur at Sonegaon towards the end of this period (Archaeological Survey of India 1965). The province produces the earliest evidence so far for rice cultivation. Rice husks and spikelet impressions on pottery and clay are reported at both Lothal and Rangpur IIA (that is, probably towards the end of the Harappan period) (Ghosh and Lal 1963:168). Rice (*Oryza sativa*) is again reported in phases II-IV at Navdatoli (*c*. 1600-1200 B.C.) (Vishnu Mittre 1961:18-19); and spread to the south to appear at Hallur in an early Iron Age context (*c*. 900-800 B.C.),[1] and yet later in Iron Age graves from the Tamil coastal plains. Rice husk impressions are not infrequently encountered in sites of the Gujarat coast during the second millennium B.C. (personal observation), but these have not yet been studied. Rice is further reported at Kaundinyapur in Maharashtra (*c*. 500 B.C.) (Vishnu Mittre 1966). From the peninsula comes the first evidence in India of another important group of cereal crops. At both Hallur and at Paiyampalli grains of finger millet (*Eleusine coracana, rāgī*) are reported, in the former case dating to about 1800 B.C.[2] Another grain from Rangpur III (*c*. 1200 B.C.) has been identified as possibly the bulrush millet (*Pennisetum typhoideum, bājrā*) (Ghosh and Lal 1963:169), and the same millet has been tentatively identified at Hallur, although in both cases there is a possibility that it may rather be *Paspalum scrobiculatum, kodon*.[3] Firm evidence of both the bulrush and finger millets, as well as of *kodon*, comes from Nevasa at the opening of the Christian era, from where too comes the earliest archaeological occurrence in India of the great millet (*Andropogon sorghum*), evidently a late arrival upon the scene (Sankalia *et al*. 1960:529:30). Kaundinyapur also yields the first evidence of maize (*Zea mays*) in the subcontinent, in the form of grain impressions on pottery. It is not clear from the published sources what is the date of its occurrence, one source referring it to a house of the Bahmani period (*c*. fifteenth century A.D.), another assigning the type of pottery to the eighth to ninth century A.D. (Vishnu Mittre 1966:152-6; Vishnu Mittre and Gupta 1966).

2. *Leguminous plants*. The peninsula also provides the first evidence of the cultivation of several species of leguminous plants. From Tekkalakota I (*c*. 1800-1600 B.C.) and Paiyampalli come examples of horse gram (*Dolichos biflorus*),[4] and from the latter site and from Navdatoli comes green gram (*Phaseolus radiatus*) (Vishnu Mittre 1961:21-2). Navdatoli yielded a very rich series of plants from its chalcolithic levels, including black gram (*Ph. mungo*), lentil (*Lens culinaris*), the grass pea (*Lathyrus sativus*), the field pea (*Pisum arvense*), and several other leguminous weeds (Vishnu Mittre 1961:20-3). Evidently another late arrival in India was the chick-pea or *canā* gram (*Cicer arietinum*), which first appears in period IV at Nevasa (third to first century B.C.) (Sankalia *et al*. 1960: 529-30).

3. *Oilseeds*. The only early oilseed so far reported in the peninsula is linseed (*Linum usitatissimum*) which occurred throughout the occupation at Navdatoli (Vishnu Mittre 1961:23-4). That one of its uses may have been to prepare flax may be inferred from the discovery of spun fibres in a string of beads from a burial at Chandoli (*c*. 1400-1200 B.C.) (Gulati 1965: 195-201).

4. *Fruits*. The Indian jujube (*Zizyphus jujuba, ber*) is reported at Navdatoli; and the myrobalan (*Phyllanthus emblica, āmalaka*) at the same site (Vishnu Mittre 1961:24-5). Whether either of these were strictly cultivated is open to doubt. Wood of the date palm is probably reported at Utnur, and date palm matting impressions also at Tekkalakota, but no date stones have been reported.[5] In modern times the date is cultivated, in the Deccan, rather for its juice which is fermented to make *tādī* than for its fruit.

5. *Other crops*. The cultivation of cotton in the peninsula is first attested by the second half of the second millennium at Nevasa, where a thread of silk and cotton was discovered in a string of beads from a burial (Gulati 1961:53-9). I have also recently noticed textile impressions, apparently of cotton, on a number of early Iron Age sherds from southern Mysore.

NORTHERN OR GANGETIC PROVINCE

This province coincides with the alluvial plains of the Ganges system. It witnesses a marked increase in rainfall and corresponding moderation of winter cold as one moves eastwards to the delta. Although much excavation has taken place comparatively little plant evidence has so far been reported.

1. *Cereals.* Only one grain has so far been discovered, rice. At Hastinapur and at Noh rice is found during the period of the Painted Grey ware (*c.* 1000-500 B.C.).[6] My own observation suggests that rice husks were frequently added to pottery of this and the following period in the central Ganges valley. Rice is again reported at Sonpur (*c.* 800-600 B.C.) (Archaeological Survey of India 1961:5) and from the western margins of the delta at Mahisadal and Pandu Rajar Dhibi already before the arrival of iron (i.e. prior to 700 B.C).[7] The importance of rice for the economy of North India is evident from the frequent references in later Vedic literature, from the time of the Yajur Veda Saṃhita onwards, and by the opening of the Christian era Susruta distinguishes no less than thirty-nine varieties (Prakash 1961:262-3).

2. *Other crops.* There is as yet no other evidence for other cultivated plants in this province.

The evidence so far assembled prompts one to form a number of tentative conclusions both on the plants themselves and on the related subject of their discovery, study and preservation. As far as I am aware there is no single center, or group of centers to which this material may be sent and where it may be studied and preserved. Dr. Vishnu Mittre of the Birbal Sahni Institute at Lucknow has been responsible for many of the published identifications, and it would seem that this center might well form a national home for archaeological plant remains of this kind. It is evident that not all field archaeologists are equally interested in collecting this sort of material, and it is to be hoped that its considerable importance may become generally appreciated. A number of the principal grains and plants found in the Indus province, and even in adjacent parts of the peninsula, appear to have been first domesticated in West Asia and to have entered South Asia probably along with the spread of cultivation in subsequent centuries. On the other hand, Professor Sir Joseph Hutchinson has pointed out to me the probability that certain of the millets, notably *Eleusine* and *Pennisetum*, were first domesticated in Africa.[8] It will be interesting to enquire whether they reached the Indian peninsula by land (in which case it may be expected that traces will be found in intermediate areas) or by some other route, perhaps by sea. Several of the plants are almost certainly indigenous. Among those we notice rice, cotton, sesamum, and Indian mustard. There is regrettably no archaeological evidence as yet for the antiquity of a number of other important Indian plants, and it is still necessary to rely upon the evidence afforded by literature. Among these plants we may cite sugar cane, first mentioned in the Atharva Veda (before 800 B.C), and the black pepper (*Piper nigrum*), for which the earliest references are in the *Āpastambha Dharmasūtra*, probably before 500 B.C. Archaeological evidence is still awaited for a number of other cultivated plants, for example yams (*sūrana*) for which the earliest literary evidence is in Caraka (first to second century A.D.). There are, however, as we have seen, several instances in which evidence so far available appears to support the literary record. For example, respecting the relative lateness of the introduction of *Cicer arietinum*, for which the earliest references are in *Rāmāyana Uttarakāṇḍa Baudhyāyana Gṛhya Seṣa-sūtra*, and *Susruta saṃhita* (suggesting collectively dates between the first and fourth centuries A.D.): or of Sorghum millet, whose earliest references are in *Caraka, Bhela* and Kaśyapa *Saṃhitas* (suggesting collectively a similar range of dates). The new perspectives provided by archaeology hold out great hopes for a reappraisal of other types of evidence. It is evident that much useful work could still be done in plotting the modern dialect names for different species and considering them in a historical light. Such "linguistic palaeontology" is likely to yield useful information, now that the chronological dimensions are beginning to appear.

NOTES

[1]Information courtesy of Dr. Nagaraja Rao.

[2]For Hallur, information courtesy of Dr. Nagaraja Rao, and for Paiyampalli information courtesy of Sri S.R. Rao.

[3]For Hallur, information courstey of Dr. Nagaraja Rao.

[4]Information courtesy of Dr. Nagaraja Rao and Sri S.R. Rao.

[5]For Hallur, information courtesy of Dr. Nagaraja Rao. The

observation for Utnur was our own, and has not so far been substantiated by further examination of specimens.

[6]See Chowdhury and Ghosh in Lal (1955:120-35). For Noh see *Indian Archaeology, A Review* for the years 1963-64 and 1964-65.

[7]For Pandu Rajar Dhibi see Das Gupta (1964:14). For Mahisdal see *Indian Archaeology: A Review*, 1963-64.

[8]On the Origin of *Eleusine coracana* see Mehra (1963).

Part VIII The Indus Script

Editor's Introduction

There can be little doubt that the literature on the system of Indus writing is at best of uneven quality. In fact, most of the books and articles which have attempted to cope with the script can be largely ignored without damage to serious scholarship. Yet it must be realized that the eventual decipherment of the Indus inscriptions is likely to yield insights into the civilization which will not be readily available from more purely archaeological contexts. Thus there must be an emphasis placed on the few examples of high quality scholarship which would seem to shed some light on this material even if we have not yet been presented with a comprehensive decipherment.

This part begins with a review paper by Arlene Zide which surveys the literature on the Indus script in considerable detail. Her basic bibliography will be of use to those interested in the history of this problem.

The two articles which follow Zide's are both modest attempts to deal with selected aspects of the Indus writing in a rigorous way. In the first, I. Mahadevan may have given us a certain translation of at least one Indus character. But of greater importance is his method. Particular attention should be paid to the manner in which he addresses the problem of translation and his rigorous standards in adhering to it. His is a promising approach and ought to be expanded to the remainder of this inscriptional corpus. B.M. Pande's paper has a similar set of modest objectives. His task is to deal with a specialized body of inscriptional data, the copper tablets, and to do the kind of tabulation and contextual studies which must precede any sound decipherment. Again, the paper has been included here as a model of sound work.

Some may be troubled by the numerous assumptions which are inherent in most studies of the Indus script. Of particular importance in this regard is placing the script within a language family. Most research proceeds on the assumption that the Harappan language will be eventually tied to some aspect of Dravidian. Of some importance in this regard is David McAlpin's recent work which gives some evidence for us to believe that in antiquity there was a Proto-Elamo-Dravidian language family which covered all of southern Iran and extended into the western portion of South Asia (McAlpin 1975). The culture historical implications of such a linguistic tie would seem to be important and far reaching.

Walter A. Fairservis, Jr. has recently published a monograph dealing with certain aspects of the Indus script (Fairservis 1976). Because of its length it could not be reprinted here, however, he has interesting and important things to say about the iconography of the Indus seals and the positioning of individual characters in the inscriptions. This evidence suggests strongly that the writing on the square, steatite seals found at Harappan sites is closely connected with a system of rank and title. His is a yet another recent approach to this body of written data which because of its rigor deserves the close scrutiny of any who would attempt to make progress toward the eventual decipherment of this script.

A Brief Survey of Work to Date on the Indus Script

ARLENE R.K. ZIDE

The discovery of the cities of the Indus Valley Civilization in 1921 and 1922 by Banerji and Sahni at Harappa and Mohenjo-daro opened up a new chapter in the prehistory of man in India. The extensive excavations by Sir John Marshall and Ernest Mackay unearthed, among other things, about 800 seals of steatite, and many copper plates incised with an unknown script of a pictorial, but, clearly, fully developed nature.

The script consists of approximately 300 signs, which can be divided into basic signs, about 250 in number, and various additional auxiliary marks which do not stand alone but which are used in combinations with the basic signs. These combinatory marks may be determinatives, vowel marks, punctuation, or other indications of gradation but it is not possible to determine at this point which analysis is correct. In addition to these signs, there occur many compound signs which are made up of the basic signs combined with each other, or basic and auxiliary signs combined. The number of signs has been variously presented as ca. 250 to ca. 375 in number; the discrepancies occur as a result of the manner in which one regards the auxiliary and compounded signs, the correct assignation often being a moot point. In any case, the number of signs, regardless of these various interpretations, would indicate that the script cannot be alphabetic, nor is it likely to be merely syllabic, but it is most probably assignable to a category which is known as logo-syllabic, i.e., composed of logograms ("ideograms") which are used syllabically as well as logographically. Thus, if we employed our English logogram 2- pronounced *tu* with a logogram for "day" to produce 2- "today" i.e., *tu-dey* in *rebus* fashion, this would be a syllabic use of logographic symbols.

In evaluating the attempts at decipherment of the Indus Valley script (or of any script, for that matter), one must bring into operation a series of criteria by which one may fairly and carefully judge the validity of the conclusions. These criteria involve basically two aspects of the decipherment; (*a*) the procedural and methodological evaluation of the script itself, and (*b*) in the event of comparison with other extant scripts, the methodology of the comparison.

Thus, a would-be decipherer must have a thorough knowledge of (*a*) the historico-geographical area and its cultures, (*b*) the field of grammatology and (*c*) the structure and typology of writing systems. His procedures must include assessment of the statistics of the script. A count of the signs yields some of the most valuable information about the structural nature of the script itself. It is known, for instance, that an alphabetic system would have 50-130 signs, and a logographic, or more properly, a logo-syllabic system, since there are no "pure" systems, would generally be in the neighbourhood of 400 or more signs, usually around 600 signs. It has been shown that even the highly logographic system usually attributed to Chinese is based on a system containing 540 "keys," or basic signs from which the vast number of Chinese signs are composed.

Further, the decipherer must make sign-lists, in order to establish the frequency and order of signs which yield important information about the graphotactics—the relevant position and sequence of the signs. In addition, word-division should be established, and numbers, being easy to recognize, must be assessed.

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In the comparison of scripts, the oldest or fullest forms should be used, and the originals from which they are derived must belong to the types of the same period; irregularities must be supported by analogies from other cases where foreign alphabets have been borrowed, and where the characters show considerable differences from their supposed prototypes, changes must be shown to have been made according to fixed principles.

Comparisons of pictorially represented writing, especially, must conform to these criteria, since the comparison of outer form is for the most part useless; pictorial representations would necessarily have to have many points of resemblance, since the objects which are represented are basically the same for all people. "Thus, men, and parts of the body, animals and plants, tools and weapons, buildings and structures, sky, earth, water and fire are everywhere represented by pictures characterized by great simplicity in form. . . . There is no need to claim for these signs one single origin" (Gelb 1952:218).

The civilization of the Indus Valley has been dated as spanning approximately 500 years, though there have been some who would assert that a shorter chronology of about 3-400 years would be more in keeping with the archaeological evidence (Misra 1967 and A. Ghosh 1964).

S.N. Kramer would place the upper limit at 2800 B.C. on the basis of evidence from Susa and Kish (Kramer 1964), since it would have taken several centuries for the Indus civilization to grow to the size it had become by 2500 B.C. but removed the date now to 2250-1700 B.C.

The chronology of the Indo-Aryan civilization in India on the other hand, has as its upper limit the date 1200 B.C., but recent reexamination of the literary evidence of the *Rig Veda* and *Atharva Veda* would place the limit as more probably around 1100 B.C., or even 1000 B.C. The archaeological evidence as well points to such a conclusion, and to a definite break between the Indus Valley civilization and the advent of the Aryan invaders (Inden 1968 and A. Ghosh 1964).

Most of the attempts to decipher the script must be considered to be invalid because of their lack of attention to the criteria as outlined above. Many of the attempts lacked completely in sound, rigorous methodological approach; others were unsuccessful because of their complete inadmissability on the grounds of chronological incongruity. Thus, any attempt to interpret or read the seals of ca. 2300 B.C., or possibly earlier as a form of Sanskrit (even "archaic" Vedic), as proposed by such would-be decipherers as Pran Nath, Ray, Gadd, Karmarkar and so on, is immediately discredited on chronological grounds.

Similarly, attempts to connect the script of the Indus Valley with the symbols of the Tantric Code (as for instance, do Barua and Sankarananda) fail to take into account, the great discrepancy in time lapse between the end of the Indus Valley culture, and the emergence of the Tantric Code many centuries later. In addition, one should note that the symbols and terms used in the Tantric texts are extremely ambiguous and obscure in nature; thus reliance on a theory of connection based on the Tantric "code" would be, at best, tenuous.

Hrozny's "decipherment" of the Indus Valley script as Hittite, is invalidated for chronological reasons, aside from his obvious lack of sound procedure. As Diringer has pointed out, Hrozny's attempt is largely based on untenable strings of hypotheses, and unsupported revisions of chronological data, proposing a derivation of the Indus Valley script from the Hittite hieroglyphics of at least 1000 years later.

The contention by Waddell that the early Sumerians were Aryans and that the Indus Valley people were early Sumerians (i.e., "Vedic Aryans" *sic*) is another excellent example of the disregard of chronological data. Further, his methodology is highly questionable, to say the least. His derivations of English, Greek, Hindi and Sanskrit words from Sumerian betrays a lack of linguistic sophistication as to the nature of reconstructions and the need for rigorous, scientific method in accomplishing a decipherment.

Heras, similarly, betrays a certain amount of linguistic nai'vete in his reconstructions of "proto-Dravidian" as the language of the Indus seals. Although his notion of chronology is far more sophisticated than that of the preceding authors, his reconstructions of "proto-Dravidian" are not scientifically credible, since they are not based on sound historical and comparative techniques. His "proto-Dravidian" bears far too close a resemblance to Old Tamil than would be likely for a language of the (projected) age of "proto-Dravidian." Proto-Dravidian is probably one of the more likely languages to be considered as a candidate for the language of the seals of the Indus Valley, since the chronological and archaeological data would not discredit such an hypothesis, at least. But Fr. Heras' "decipherment" leaves much to be desired in the way of scientific accuracy and metho-

dological procedures. Lacking a solid reconstruction of "proto-Dravidian" forms, a scientifically and linguistically accurate notion of the structure of the writing system, and rigorous systematic procedures, Heras' "decipherment" cannot be taken seriously. The comparison of scripts involves many pitfalls as well. Thus, the comparison of the Indus Valley script with the so-called "script" of Easter Island by de Hevesy, taken up later by others such as Heine-Geldern and Bilimoria is a striking example of fuzzy thinking. The Easter Island script is not a script at all. Unlike a fully developed script, whether alphabetic, syllabic or logographic, the Easter Island "writing" cannot be statistically assessed. Thus counts ranging from 120 to several thousands have been made for it. The range of variation of a sign is enormous, pointing not to a modicum of simplicity usually associated with a writing system which after all is presumably used for the purpose of communication, but to at most, semasiographic, pictorial representations which cannot be called a "system" at all. The distribution of the signs and the lack of groups exclude the possibility of phoneticism and the limited number of different symbols eliminates the possibility of the Easter Island writing being a pure pictography.[1]

The 200 or so signs which de Hevesy supposedly connected with the Indus signs were shown by Alfred Metraux, to be mostly inaccurate representations, with those resemblances remaining, being only of the oddest sort, not of the most characteristic signs. "Hevesy fails to explain how 2 scripts separated in time by 4000 years at least, can present minute and complicated resemblances in trifling details and at the same time be so completely different in all the essential elements." In his comparison of signs he chooses from the thousands of Easter Island signs small variations which appear once or twice, thus violating the principles of valid comparison of scripts as outlined above. Further, here again chronology would indicate the impossibility of connection of two "scripts" over a period of at least 4000 years, preserved throughout the vicissitudes of wars and migrations, only to reappear on a Spanish war of the 19th century.

Another example of faulty comparison is that of Piccoli (1933) who compared 40 isolated signs on Etruscan utensils with simple geometrical signs found in the Indus script. Like all comparisons of this sort, resemblances are bound to appear; but the signs of the Indus Valley compared are just the widespread variety basic to any pictographic or even syllabic script, e.g., circles, plusses, a zigzag line, a square with a plus in it, etc. Nothing conclusive could conceivably be inferred from such scanty evidence of identity. Further, of the various unknowns, Etruscan is an unknown language which is in a known writing, thus these marks are presumably potters' or owners' marks and not scripts at all. The validity or even usefulness of a proposed connection between such diverse civilizations, based on the doubtful "evidence" of identity of a few minor, widespread, uncharacteristic signs is to say the least, questionable.

There have been two attempts at what might be termed "interpretation" of the seals, rather than actual decipherment. Readings are made without the actual reduction of the signs to phonological elements, or words of a particular language.

The treatment by Meriggi of the Indus Valley script is one of these interpretative attempts. Meriggi classifies the script as ideo-phonographic, presumably by this term he means logo-syllabic, and attempts to classify the signs of the inscriptions into determinatives, ideograms, *sic* etc. He asserts that the script most closely resembles the Hittite hieroglyphic but does not go so far as Hrozny in identifying the language of the inscriptions as Hittite, or even Indo-European. Thus he does not postulate any phonetic values for the signs, and his work must come under the category of preparatory work towards a decipherment, rather than any actual attempt at decipherment.

Like Meriggi, Petrie interprets rather than deciphers the seals of the Indus. His interpretation is based on proposed similarity of the writing of titles in Ancient Egypt to what Petrie contends are the administrative seals of the Indus Valley. A major objection to such an interpretation of the function of the seals, as merely seals of the officials of the government, is that there would have been as many officials as there were people in the Indus; often several officials per family, since several seals were often found in the same rooms and houses. Though it is certainly plausible that many of such seals were those of contemporary officials of the government, not all could have been, unless they were hereditary, possibly titular seals of authority, which passed from generation to generation.

The interpretation of these seals as administrative seals as do Meriggi and Petrie results in far too many administrative seals; if this defective theory is correct, then everyone in Mohenjo-daro for instance, was an official of the government.

Both Hunter and Langdon refrain from claiming

that they have deciphered the script, and Hunter in particular, should be commended for his methodical, scientifically researched lists and tables of signs. They both, however, would assert the connection between the script of the Indus Valley and the much later Brāhmī script. At the point Hunter argued for this connection, part of his assumption was based on the lack of knowledge of the lower limits of the Proto-Indian civilization. These limits are far clearer now and would thus militate against such a proposed connection because of the great gap in time. In addition, as Mr. Shapiro has shown, Brāhmī is derivable (as a consonantal skeleton with vowel markers) from North Semitic and is in structural character far removed from the Indus system, which is clearly logo-syllabic.

There have been several serious and successful attempts at treatment of the Indus script. Those treatments which could be considered valid, however, are not full decipherments, or even interpretations of the scripts, but sound conclusions about various aspects of the system based on clear, demonstrable principles, often with analogies from the known writing systems of the world. Thus, the work by B.B. Lal of recent years on the direction of the script, as based on discernible overlaps of incised signs on sherds, and the analysis of the possible numeral signs by A. Ross should be noted for their adherence to sound procedure.

Ross is the only author who makes inferences from the number of different signs and one of the handful who employs scientific, evaluative procedures rather than rhetoric in his examination and conclusions about the nature of the script, or as in his particular treatment the nature of the numeral system. His conclusion is that the chief numeration-system is decimal, i.e., that the base of the script itself is greater than 8 and not 11, and that 12 is not a change-point but is specifically indicated. Further, that the chief function of the numeral-signs appears to be a syllabic one, based on position of sign-groups and the proximity of certain numeral signs.

Lal's treatment indicates without doubt that the direction of writing of the Indus Valley inscriptions is from right to left, as has been heretofore suspected but never conclusively demonstrated. Overlap of incised signs, and the careful evaluation of a seal, inscribed along three edges, furnish the concrete proof for this assertion.

Hunter also, as indicated above, aside from his untenable derivation of Brāhmī from the signs of the Indus Valley, has presented an excellent and full set of tables and lists, and evaluation of sign-groups, based on sound principles.

Smith, in his chapter on the "Mechanical Nature of the Early Indus Writing" in *Mohenjo-daro and the Indus Civilization* makes a careful and useful evaluation of the structure of the writing system and the stratification of the seals, though correctly, he draws no conclusions from this early, brief study.

In summary, we may conclude that for the most part, the attempts at the decipherment of the inscriptions on the seals of the Indus Valley civilization have been unsuccessful; except for a handful of methodic evaluations as outlined above, the "decipherments" have been based on untenable hypotheses, lack of chronological accuracy, unsound reconstructions because of linguistic nai'vete, and in general, preconceptions of the language of the inscriptions which were not based on any systematic or rigorous evaluation but on *a priori* and untenable assertions. The linguistic principles of decipherment as presented in the early part of this paper were ignored or violated.

The script of the Indus Valley seals as we have it, one must conclude, is virtually undecipherable. The task of deciphering an unknown language in an unknown script is, to say the least, formidable. The inscriptions which are left to us, are far too short, and too limited in nature to infer any readings; further, the names of the civilization are lost to us, and as names usually provide the keystone of a decipherment, the possibility of breaking the script is extremely unlikely. What can be inferred about the script, is limited to certain structural conclusions such as its nature as a type of logosyllabic writing, the direction of the writing, and possibly the numeral-signs as outlined by Ross. A thorough evaluation of sign-groups incorporating the newer material from Lothal and Kalibangan would be a useful study preparatory to a possible later decipherment. Its connection with another known script is possible, but, as it is a pictographic script, conclusive rigorous demonstration would be necessary to prove that the resemblances were more than the predictable fortuitous ones, expected between any two pictographic scripts representing similar objects.

What is needed for a successful decipherment of the script are either bilingual texts, or longer inscriptions containing names, or place-names which could be identified and thus provide a key to syllabic values of certain of the signs.

For such aids, I believe, we must look to further excavations not in the Indus sites themselves, but in those of the earliest Sumerian, Ubaidian, and Elamite remains in Iran and places such as Susa, Jemdet-Nasr, and Kish where it is clear there is contact with the Indus Civilization at the early stages. Further, if, as Dr. Dales has suggested (1967), the lack of longer texts should be attributed to the use of perishable writing materials such as wax, we cannot hope to find longer inscriptions in India or Pakistan. Until such aids to decipherment are found, the most useful research on the Indus seals would be a careful preparatory compilation, perhaps by computer analysis, of all the sign-groups of seals including those from the recent excavations at Kalibangan, Lothal and other sites in India.

NOTE

[1]That is, in Gelb's terminology a *forerunner of writing*, using pictures as signs, e.g. as among the American Indians or Eskimos (Gelb 1952:250-251).

Study of the Indus Script through Bi-lingual Parallels

IRAVATHAM MAHADEVAN

The Background

During the half-century which has elasped since the discovery of the Harappan civilization, three developments have taken place which have greatly increased the probability that the civilization was Dravidian.

In the first place, new discoveries have revealed the great extent and duration of the Harappan civilization and have led to a fundamental re-assessment of its character. The spade of the archaeologist has uncovered hundreds of Harappan sites over a vast area from the eastern borders of Iran to the Gangetic Doab and from the foot-hills of the Himalayas to the estuary of the Tapti. In the Gujerat region, late Harappan, sub-Harappan and post-Harappan settlements occur in sequence and demonstrate the survival of the Harappan influence well after the middle of the Second Millennium B.C. What was thought to be a localized culture of the "Indus Valley" (which, at least by implication, could have been created by relatively small bands of alien maritime people) has now turned out to be the largest Bronze Age civilization known to the ancient world. It has now become inconceivable that this great and populous civilization, with its continental spread and millennial duration, should have appeared suddenly or utterly perished without a trace. Ethnic continuity overlaid by a linguistic change wrought by the incoming Aryans seems to be the only possible answer to the question, "What happened to the Harappans?"

Secondly, recent advances in Dravidian studies have led to an increasing realization of the decisive influence of the Dravidian substratum over the evolution of the Indo-Aryan languages and Hindu social institutions. It is now well established that the Dravidians were present in North-west India when the Aryans entered the country, most probably sometime around the middle of the Second Millennium B.C. The survival of the Brahui, a Dravidian language, and the presence of words of Dravidian origin in the Rigveda, provide irrefutable evidence for this fact. While the Aryans imposed their language and established a new social order, they themselves must have been in a small minority and rapidly lost their ethnic identity. So complete is the racial fusion, that the terms "Aryan" and "Dravidian" can now be used legitimately only in a linguistic context. While the Dravidian languages have disappeared over most of North India, their substratum influence on the Indo-Aryan languages is most clearly seen in the latter in phonological changes like the introduction of retroflex sounds, in morphological changes like the switch-over from inflexion to post-fixation, in lexical borrowings, and especially in the near-identical syntactical structures of the modern Indo-Aryan and Dravidian languages. Such changes could have been brought about only after an extended era of bi-lingualism particularly on the part of the subject people (Dravidian speakers, in the present instance), as always happens in similar circumstances.

The racial assimilation of the Aryans by the Dravidian people led in due course to the re-emergence of Dravidian social institutions, although with a Sanskritic veneer. In the field of religion, the older Dravidian deities like the Mother Goddess and Dravidian modes of worship pushed the Vedic religion into the

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background even by the time of the evolution of the middle Indo-Aryan dialects. Our increasing knowledge of the extent of the non-Aryan and Dravidian substratum influence in North India, has made the earlier view of some historians that, upon the advent of the Aryans, the indigenous population retreated southwards and that those who remained back were made into slaves and serfs, much less probable. There were undoubtedly migrations and subjugation in the earliest phase; but the numerical and cultural superiority of the indigenous population make it unlikely that they were all driven away or relegated to the lowest rungs of the society. The transformation of the Hindu religion in the post-Vedic period would have been impossible but for the fact that the new classes of priests as well as of kings, nobles and merchant-princes who patronized the priests, had risen from the indigenous non-Aryan stock. This circumstance also makes it probable that at least a part of the historical tradition of ancient India recorded in the Epics and the Puranas or handed down as ballads and folk-lore may go back to the pre-Aryan epoch.

Thirdly, systematic studies of the Indus Script, using scientific methods of statistical-positional analysis have led the investigators to the conclusion that the Harappan language is typologically non-Indo-European and resembles the Dravidian languages closely. In particular, it appears that the Harappan language was mono-syllabic and of the suffixing type with a Dravidian-like word-order.

When we place these developments side by side and consider objectively (*a*) the extent and duration of the Harappan civilization, (*b*) the extent and character of Dravidian substratum influence in North India from the Vedic Age, and (*c*) the Dravidian-like typological features of the Indus Script, we cannot but be struck by the inevitability of their inter-connection. To hold otherwise would be to presume that the extensive Harappan civilization left no discernible traces and that the deep Dravidian substratum influence is totally unmatched by any material remains. Neither assumption seems to be reasonable in the light of our present knowledge of the linguistic and social pre-history of India. It has also been argued that the possibility of the Harappan language being typologically similar to, but not necessarily identical with Dravidian, cannot be ruled out. The evidence of the new developments summarized above makes this view altogether too cautious and even somewhat pedantic. Taking the totality of available evidence, the hypothesis of a Dravidian authorship of the Harappan civilization seems to offer the most promising line of investigation. Absolute certainty can however be reached only when an acceptable solution to the riddle of the Indus Script emerges in the fullness of time.

The Method

When I began working on the Indus Script four years ago, I had confined my attention almost exclusively to the Dravidian parallels—old Tamil literature and inscriptions, Dravidian syntactical patterns, typology of Dravidian names, etc. I am still of the view that it is essential to look for Dravidian parallels in view of the strong probability of the Harappan civilization being Dravidian. However, the preliminary findings published in my paper "Dravidian Parallels in Proto-Indian Script" (*Journal of Tamil Studies*, II:1, April 1970) evoked two kinds of constructive criticism. In the first place, it was pointed out that it would be necessary to find evidence to bridge the enormous gap in time and space between the end of the Harappan civilization and the earliest records of Dravidian culture in South India. Secondly, some method is needed to provide a check on the proposed readings and interpretations. It was while pondering over these problems that I hit upon the method of tackling the Indus Script with the aid of bi-lingual parallels drawn from both the Indo-Aryan and the Dravidian languages. The theoretical possibility of such an approach suggested itself to me from considerations of the historical background summarized in the first part of this paper.

The method of bi-lingual parallels is based on three crucial assumptions:

(1) The Harappan seals, in accordance with universal usage, give the names of their owners. The longer texts probably also contain titles, honorifics, references to occupations, place-names and other ancillary information. It is likely that due to extended bilingualism and racial fusion, some at least of the more important names and titles (as judged by their frequency on the seal-texts) passed into the Indo-Aryan languages as loan-words, loan-translations or hybrid translations.

(2) It is also possible that when the Indus Script disintegrated as a writing system at the end of the Harappan civilization, at least some of the signs consisting of the more important ideograms and phonograms (again as judged by their frequency)

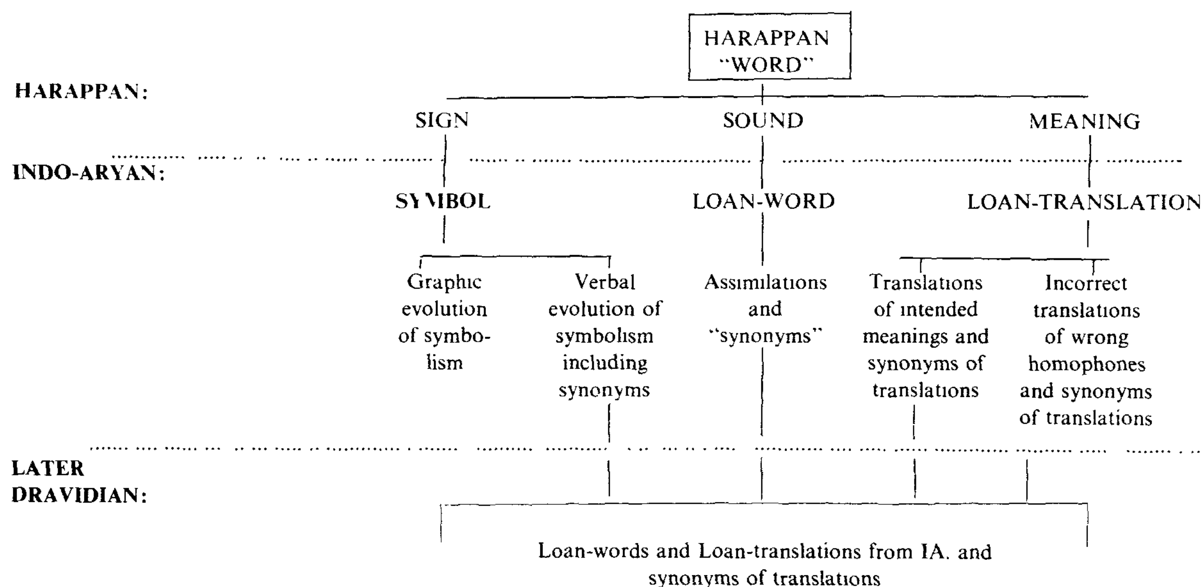
survived and evolved into traditional symbols of various kinds. Such symbols may consist of iconographic elements and other religious symbols, royal insignia, emblems on coins and seals, heraldic signs of the nobility, corporate symbols, totem signs of clans and tribes etc. It is likely that the symbols were continued to be associated, even though in a conventional manner, with the new forms of the same names and titles which were earlier represented by the corresponding signs in the script ideographically or homonymously.

(3) Such survivals of names and associated symbols can be recognized by (*a*) the arbitrariness of the symbolism, (*b*) the absence of a convincing Indo-Aryan etymology for the loan-words, (*c*) the tell-tale presence of myth and folk etymology invoked to explain the symbolism and the loan-words, and (*d*) the clues furnished by the inevitable distortions that ideas and meanings undergo on transfer from one socio-linguistic context to another.

The method opens up, in principle, a promising avenue for the exploration of the Indus Script. It should be possible to undertake a comparison of the traditional symbols resembling the signs of the Indus Script and the names and concepts associated with the symbols in Indian historical tradition in an attempt to establish the original ideographic meanings of the symbols and, in especially favourable circumstances, even to suggest their phonetic values. The results thus obtained should not be inconsistent with those suggested by statistical-positional analysis of the signs and the sign-combinations.

The application of the method in actual practice is rendered difficult by an incredibly complex evolutionary situation. The traditional symbols (derived from the Harappan signs) may undergo both graphic and verbal evolution, acquire new significance, and even become unrecognizable pictorially or be replaced verbally by synonyms. The loan-words (borrowed from the Harappan language) may be assimilated to phonetically similar words in the borrowing languages and may be substituted by "synonyms." The loan-translations may not be faithful to the originals and may be based on the wrong homophones. These translations may also get replaced by synonyms in due course with attendant distortion in emphasis or shades of meaning. These developments are shown schematically in the chart.

The situation is in fact much more complex than is suggested by this neat little chart. The continental size of the country and the great time-depth (not less than three millennia) involved in the changes and the incredible diversity of the Indian society have also to be taken into account. One has therefore to be on the look out for *layers* of parallelisms as well as different, and often divergent, *streams* of parallelisms. As a result, the Harappan sign, word and its meaning may be reflected in Indo-Aryan and in later tradition, not



by just one set of symbol, loan-word and loan-translation respectively, but by multiple sets of symbols and words, occurring in different regions and at different periods, all differing from one another, but all of them based on the same proto-type.

The two great linguistic traditions of India, Indo-Aryan and Dravidian, continually acting and re-acting upon each other, add yet another dimension to this picture. We saw that the Indo-Aryan languages were influenced by the substratum Dravidian languages and moved closer to the latter in many ways. But we should not forget the much greater influence of the dominant Sanskritic tradition on the Dravidian languages in historical times. In this situation it could happen that ideas which originated in Dravidian in the Harappan age, and which were borrowed by the Indo-Aryan at a very early period, travelled back to Dravidian at a much later time. In such cases it is almost certain that the secondary Dravidian concepts and words would not restore the primary Dravidian values of the earlier epoch.

Finally, the method itself is severely limited by its dependence on accidental and random survivals of symbols and words and the uncertainty in recognizing loan-words and loan-translations.

In spite of these uncertainties and limitations, which I have deliberately emphasized here, the method of bi-lingual parallels would seem to be the only one available at present for at least a partial understanding of the context of the Harappan texts. The discrepant traditions themselves can provide valuable clues. I feel that, in favorable circumstances and interpreted with due care and circumspection, bi-lingual parallels based on concrete Harappan signs, can be virtually as good as bi-lingual inscriptions.

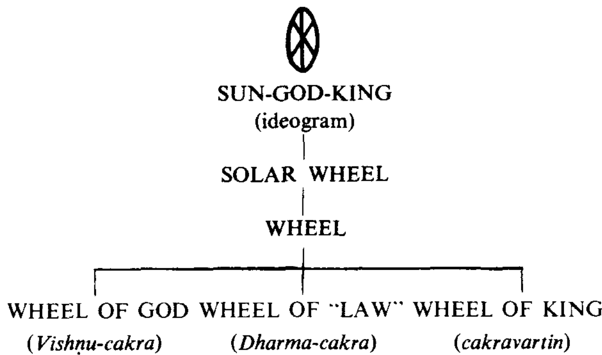
Some Bi-lingual Parallels

In this concluding section, I shall give some examples to show how bi-lingual parallels work. To present the entire evidence I have collected would require monographic treatment. Here I shall have to be content with a few typical cases to illustrate the various theoretical aspects of the method discussed in the earlier sections of the paper. There will be no emphasis on any individual case, each of which represents no more than a possibility with varying degrees of probability. However many of the parallels collected by me turn out to be inter-linked and, in sum, present a coherent picture. In particular, the correspondences between sign-combinations and names and titles known to later Indian tradition are quite interesting and seem to indicate the basic soundness of the method.

The "WHEEL" sign:

An easily recognizable Harappan sign, which appears at first sight to be the "wheel," is a circle with six radial lines or "spokes" within. There is no evidence for a spoked wheel at Harappa or Mohenjodaro, where all the toy-carts so far found have solid wheels. However, the resemblance between the sign and the wheel is so strong that we may, as a first approximation, take the sign to represent the wheel and look for parallels. The wheel is a well-known symbol in Indian tradition, standing for two distinct but inter-related concepts, namely, divinity and sovereignty. The wheel is one of the attributes of the supreme Deity, conceived as Vishṇu or Krishṇa. In the Buddhist tradition, the wheel represents *dharma*, an abstract concept which takes the place of the supreme Deity in this creed. The wheel is also the symbol of the paramount sovereign, *cakravartin*, in both Hindu and Jaina traditions. It is therefore not unlikely that the WHEEL sign has a somewhat similar meaning in the Indus Script and is an ideogram representing some divine or sovereign entity.

The Vedic imagery of a "solar wheel" gives us the clue to the probable original meaning of the sign. Again the Vedic Mitra whose most characteristic attribute is sovereignty, is a solar deity. Thirdly, contemporary West Asian representations of the sun show striking graphic similarities. From all this evidence, it can be inferred that the Harappan sign originally represented the sun or rather the Sun-god. The Sun-god was probably the supreme Deity as well as the titular sovereign of the Harappans. The latter inference is supported by contemporary West Asian parallels, later Dravidian tradition and the complete absence of royal paraphernalia in the Harappan cities. The accidental resemblance of the stylized representation of the sun to a wheel led, after the introduction of the spoked wheel in India, to the symbolism of the solar wheel. With the eclipse of the Sun-god cult at a later date, the symbolism centered only on the wheel and it is to this last stage that we should attribute the wheel-based concepts like *Vishṇu-cakra*, *Dharma-cakra* and *cakravartin*. Schematically, the evolution of the solar-wheel symbolism can be represented as follows:



This analysis shows that the "wheel" symbolism is unlikely to be related to the original phonetic value of the sign. We can thus rule out Dravidian words based on the "wheel" symbolism as secondary borrowings from a modified Sanskritic tradition. The earliest and the nearest Indo-Aryan equivalent to the original SUN-GOD-KING concept is the Vedic Mitra, who combines in himself all the three elements. But the name Mitra ("friend") does not appear to be naturally related to any of the three elements. The primitive Dravidian expression for SUN-GOD-KING can be reconstructed with some probability as **vec/vey/vē-* from the following etyma:

SUN: *vēy-ōṉ* (O.Ta.) < *vē-*: to be hot (DED. 4540)

GOD: *vē-(n)d-id, vē-(n)d-iḍ* (Pa.) *vē-nd-iṭ* (Ga.), *vēnu* (Kui) (DED. 4550)

KING: *vē-nt-aṉ*, (the word also connotes the sun and some deities) (DED. 4549)

There are several interesting features in these results. It should be noted that the primitive Dravidian word **vē-* for "god" is confined to non-literary Dravidian tribes and could also mean "demon." It is clear that both these developments are due to the dominant Sanskritic and Brahmanical influence on the Dravidian literary languages. It will also be seen that the old Tamil *vē-nt-aṉ*, "paramount sovereign," while being the equivalent of *cakravartin* is not based on the "wheel" symbolism and is distinctly related to "sun" and "god" concepts—a clear indication of its direct descent from the original. Finally, a rather surprising result is that even the Rigvedic (and Indo-Iranian) Mitra seems to have developed from, or is in some manner related to, the Harappan-Dravidian substratum.

The duality of the tradition in respect of this sign as the "sun" and the "wheel" symbols suggests the following two pairs of interesting parallelisms based upon three "introductory" signs in the Harappan texts with very similar positional and functional characteristics:

: SUN and (CRESCENT) MOON

: WHEEL and CONCH

The SUN and the MOON signs should be compared with the legendary symbolism of the Solar and the Lunar dynasties of Indian tradition. While this pair seems to have retained the original pictographic (but literal) values, the "WHEEL-CONCH" (*cakra* and *śankha*) symbolism associated with Vishṇu, is a later development which appears to be based on accidental resemblances to the objects in question. While the matter cannot be pursued further within the scope of this paper, it can be stated that both the pairs of later parallelisms point out to the general direction of evolution of these Harappan concepts and help us to interpret other associated signs.

The "BEARER" sign:

Among the anthropomorphic signs of the Indus Script there is a frequent sign which depicts a person carrying a yoke across his shoulders with loads suspended from either end. From the frequency and the positional characteristics of the sign, it appears to be the ideogram for an important title. We can thus rule out literal interpretations like "porter" or "water-carrier," though a meaning somewhat like "servant of god/king" is still possible. The matter has to be decided not on the basis of theoretical possibilities but on the evidence of Indian tradition. There seems to be no graphic example of the "bearer" motif from later times. However, I have been able to find out a number of interesting "verbal symbolisms" based unmistakably on this concept.

It turns out that in Indian tradition the "bearer" is an idiom and refers to a person who "shoulders" any responsibility or "bears" the "burden" of any office. Take the common Sanskrit expression *bhartṛ*, "lord, master, husband," etc. It is derived from the root *bhṛ*, "to bear" and means literally "one who bears," but idiomatically "one who sustains or maintains." The Prakrit equivalent, *bhaṭṭa* is also a honorific applied,

significantly enough, to a prince or a priest. We have similar expressions derived from the root *vah*, "to bear" as in *kārya-vāhaka*, "office-bearer." Other expressions are derived from the symbolism of the yoke, as in *yugam-dhara* and *dhuram-dhara*, both literally meaning "yoke-bearer" but used as honorifics or names. On the basis of this linguistic evidence, we can interpret the BEARER ideogram as a honorific assumed by the priest-rulers of the Harappan polity with approximately the same significance as in later tradition.

A common tendency in Indian tradition is for honorifics and titles to lose their original significance and become proper names. If a similar development had taken place in respect of the "bearer" symbolism, we should find such names among the princely or priestly clans in later times. This reasoning leads us straight to the earliest and the most famous of the "bearer" clans in ancient India, namely, the Bharatas (literally, the "bearers"). Since the Bharatas were priests and rulers and known to have occupied the Indus region during the Vedic period, it is probable that they were the descendants of the priest-rulers of the Harappan civilization.

A search for other royal names based on the "bearer" motif, led me to the famous Andhra dynasty whose kings called themselves Sātavāhanas or Sālivāhanas. The suffix *-vāhana* appeared to be connected with the "bearer" theme (*vahana*: bearing, carrying). Since however the second element *-vāhana* never appeared separately, it struck me as probable that the preceding elements *sāta-* and *sāli-* might also be derived from the Harappan substratum. Now it so happens that the BEARER ideogram in the Indus Script often appears with one of two signs ligatured to it, namely, the JAR sign and the ARROW sign. I wondered whether these ligatured signs occurring with the BEARER ideogram, had anything to do with the prefixed elements found with the later *-vāhana* names. A search led to the discovery of the interesting parallelisms shown below.

Can all this be due to mere coincidence? Is it probable that three Harappan signs, given their pictographic equivalents in Sanskrit, produce by random chance, two composite historical names corresponding to two different sign-combinations in the script? I think we can rule out coincidence and conclude that these are true parallelisms resulting from the substratum Harappan influence.

The Andhras were a Dravidian people and their earliest territory lay in the north-west, not far to the south of the known southern limit of the Harappan civilization. The Andhras, though kings, claimed also to be priests ("*eka-brāhmaṇa*") and derived their descent from the Vedic sages matrilineally. It is however certain that "Sātavāhana" and "Śālivāhana" are not the original names of the Dravidian Andhra dynasty. We know definitely that the JAR and the ARROW signs in the Indus Script are suffixes and have to be read *after* the BEARER sign with which they are

Sign	*Pictorial value*	*IA equivalent*	*Meaning*
	JAR	*Sata*	A kind of (sacrificial) vessel
	ARROW, LANCE	*Śalya*	arrow, lance
	BEARER	*Vahana*	Bearing, carrying
	JAR + BEARER	*Sata-vahana*	(lit., "jar-bearing")
		> *Sāta-vāhana*	n.pr. of Andhra dynasty
	LANCE + BEARER	*Śālya-vahana*	(lit., "lance-bearing")
		> *Śāli-vāhana*	n.pr. of Andhra dynasty

ligatured. Thus the Harappan word-order was BEARER-JAR and BEARER-ARROW and not the other way about. It is probable that the Andhra chieftains, who must have been bi-lingual, assumed their Sanskritized titles on the basis of their traditional clan symbols whose original significance had been lost much earlier. In the absence of any information to the contrary, the sequence JAR/LANCE-BEARER would have seemed more natural and became the basis for the loan-translations.

There is a parallel Dravidian linguistic tradition based on the "bearer" symbolism, as may be seen from the following cognates listed in DED. 3729:

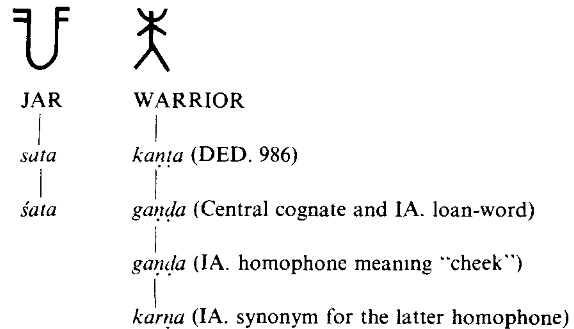
Poṟu (Ta.):	To bear, to take responsibility
Poṟai (Ta):	Load, weight
Poṟaiyaṉ (Ta):	Sustainer
Poṟıd- (Koḍ.):	To undertake an office

The names Poṟai and Poṟaiyaṉ occur as the clan titles of the Chera dynasty. The linguistic evidence presented above makes it unlikely that the title *Poṟai* is a secondary loan-translation from Indo-Aryan.

We may therefore conclude that the Bharatas in the Vedic and the Epic periods in North India, the Sātavāhanas in the Puranic and early historical periods in the Deccan and the Cheras (Poṟaiyar) in the early historical period in the Tamil country, represent different layers and streams of parallelisms, all ultimately going back to the "bearer" concept of the Harappan-Dravidian substratum.

A striking corroboration of the ultimate common origin of these peoples is furnished by the identification of *sata* as one of the later epithets for the "jar" symbolism. This word has been cited even by the ancient authors as a Dravidian loan-word and it is, in any case, little more than a lexical entry in Sanskrit. It is therefore natural that it should have been confused with *śata* (Pkt., *sata*), "one hundred," a far more frequent word. This may explain the conventional number of "one hundred" Kauravas who were also, according to legend, born in a jar! The epithet *śata* (> *śāta*) links the Kurus (who were Bhāratas) both with the Andhras and with the Cheras whose characteristic personal name Ātaṉ, seems to be a re-borrowing derived from **Cata-* by the dropping of the initial palatal. Thus the "bearer" clans and the "jar-born" clans are related and seem to be the descendants of the Harappan priest-rulers. The association of "jar" names and legends in later Indian tradition with priestly families (e.g., Vasishtha, Agastya and Droṇa) and royal dynasties (e.g., the Kurus, the Andhras and the Tamil Vēlir) is the best evidence we have for supposing that the Harappan polity was ruled by a priestly oligarchy.

The identification of *sata* as a term related to the later "jar" symbolism provides us with yet another parallelism corroborating the preservation of the Harappan symbolism by the Andhra dynasty. A frequent honorific in the Harappan texts consists of a pair of signs, viz., a HORNED PERSONAGE (an obvious ideogram for a hero, warrior or chief) followed by the very common JAR suffix. Here again, as in the case of the BEARER and JAR combination, it appears that the original significance and phonetic values were lost, but the signs survived as traditional clan symbols. This resulted in the order of the symbols being reversed in this case also in the process of Sanskritization of the term. With this assumption (for which there are quite a number of parallels), we can schematically trace the probable development of the later symbolism as follows:



Thus we get *Śata-karṇa* (>Sāta-karṇin) as a later parallelism developed out of the Harappan JAR and HERO signs. Some evidence for this development is provided by the recorded variant Śata-kaṇṭha for the name of the Andhra dynasty and the occurrence of Kumbha (another "jar" synonym) and Karṇa among the names of the Andhra kings, and Kumbhakarṇa and Karṇa as mythological names without satisfactory etymologies.

The bi-lingual parallels cited in the paper (and many others I have been able to collect) bring out the amazing continuity of the Indian historical tradition which is indeed the "Rosetta Stone" of the Indus Script. They also serve to bridge the gap in time and space between the two Dravidian periods and make the suggested interpretations of the Harappan signs more credible.

Inscribed Copper Tablets from Mohenjo Daro: A Preliminary Analysis

B.M. PANDE

Introduction

In addition to various other types of inscribed material, viz., steatite and terracotta seals and sealings, ivory sticks and potsherds with graffiti, a number of flat, inscribed copper tablets were found in the excavations at Mohenjo-daro by Marshall (1931) and Mackay (1938b). These copper tablets are either rectangular or square in shape; of these, the latter, which are rarer and measure on an average 2.3 cm. square, are thicker than the long and narrow ones whose size ranges from 3 by 1.3 cm. to 3.8 by 2.5 cm.

These copper tablets differ from the other inscribed material found at Mohenjo-daro and other Harappan sites, in that in the other mediums the inscription and the accompanying figure (if any) are on one side only, in the copper tablets, there is an inscription on one face (obverse) and an animal or human figure or a symbol on the other (reverse). This feature of having an inscription engraved in positive on one face (obverse) of the tablet and a symbol or animal figure on the other (reverse) is a characteristic of the copper tablets. In two cases, however, there is also an inscription above the animal figure on the reverse and, in four cases, an additional line is engraved above an inscription. In one case, only the figure has been engraved on one face without any inscription on the other.

Since all the inscriptions are engraved in positive and are not very deep, it is obvious that the copper tablets were not used for stamping purposes. The inscriptions, therefore, are to be read from the right to the left as they have been engraved, as has been amply demonstrated by scholars working on the subject (Lal 1966, 1967-68). In three cases (nos. 9, 67, 75), however, the order of signs has been reversed, perhaps due to scribe's error.

The number of copper tablets published by the excavators, Marshall and Mackay, is forty-three. Hunter, in his study (1934), included twenty-six copper tablets, some of which had also been included by Marshall and Mackay. Apart from these, there are some specimens in the Central Antiquities Collection of the Archaeological Survey of India and the National Museum, New Delhi, which I have incorporated in the present study. In all, therefore, 123 examples of copper tablets have been included.

In view of the non-availability of details pertaining to the find-spot, area, depth, etc., it has not been possible to examine the data from the point of view of area-wise distribution and evolutionary changes in a vertical order. However, in the accompanying figures (35.1-35.4), the copper tablets have been arranged area-wise as far as is known.

Area-wise Distribution of Copper Tablets

Out of the 123 tablets, 76 were found from the DK Area, 20 from the VS area, 13 from the HR Area, 4 from the L Area, 7 from the SD Area and of the remaining 3, the details about the area could not be ascertained. The tablets have been arranged area-wise in Figs. 35.1-35.4 on the basis of the classification of the end-signs and the details regarding find-spot etc., have been shown in the key to figures. The basis of classification of the final signs is shown in Fig. 35.5.

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Inscriptions vis-a-vis the Reverse Symbol, Animal/Human Figure

On the basis of his study, Hunter concluded that the signs used in the inscriptions on the copper tablets "are independent of the accompanying animal design" (Hunter 1934:27). While it is true that certain inscriptions are common in certain tablets having different reverse devices, it is, however, noticeable that certain inscriptions occur only with particular reverse devices (Fig. 35.6). Likewise, there are two different inscriptions with the same animal figure (Figs. 35.1-35. 3 and 35.6:8, 10, 11, 14, 15, 70-73, and 87-89). In certain tablets the obverse inscription is the same, though the reverse device is different (nos. 12, 13, 16, 79 and 80; 17-27, 29-34; 50-57). The problem of connexion between the reverse device and the inscription, however, requires further study.

The inscriptions in the copper tablets are composed of signs ranging from one to seven and originally only a single line was meant to be engraved on them. In those cases, where there is a second line (nos. 6. 7,61 and 73), it seems to have been added subsequently. However, the inscription on the reverse above the animal figure in one case (no. 14) seems to have been put originally; in another case (Fig. 35.4, no. 112), the inscription above the animal figure on the reverse seems to be a subsequent addition.

In a majority of cases, particularly in the copper tablets from the DK Area, the final-sign is the other final-signs being , , , , , and (Figure 35.5). In five cases, the end-sign is which has been included in inscriptions of Class 'A' wherein the end-sign is

In three cases, the order of signs seems to have been reversed (Figs. 35.1 and 35.3, nos. 9, 67 and 75) and in another (Fig. 35.3, no. 69), the sign which is normally the final-sign has been put first, though it does not seem to have been a part of the inscription because of the gap between the inscription and the sign. In one example (Fig. 35.3, no. 68), only one sign has been used.

Suggestions

In the present paper, I have tried to indicate a line of approach regarding the analysis of the inscriptions, notwithstanding the fact that various kinds of analyses have been done by scholars. However, all the earlier analyses were only mechanical analysis of the script and the inscriptions. For a proper understanding of the material, it is necessary to undertake a study of the inscribed objects from the point of view of horizontal and vertical context, implying thereby a study of the inscribed material (*i*) in terms of the *milieu* and (*ii*) in a sequential order. This can give a firm basis to our study of the inscriptions in general which have been studied shorn of their context. I shall illustrate this further.

To start with, area-wise distribution of the various inscribed material should be undertaken and the inscriptions in different media should be compared. I have undertaken a study of the inscriptions on the copper tablets vis-a-vis the inscriptions in other mediums and have found that, more often than not, most of the inscriptions on the copper tablets are not repeated in the other mediums. However, certain blocks, or juxtaposition of signs are common to inscriptions in the other mediums. (I propose to publish the results in a subsequent paper.)

It is thus imperative that the inscribed material in different media from each site and from different areas should be studied in relation to the inscriptions in the same and the other media and within each particular area, a sequential development in terms of changes should be analyzed. Likewise, a comparative study should be undertaken of the seals, sealings, etc. of the same shape, size and type and then compare the inscriptions in other varieties/mediums. It may also be examined as to why certain copper tablets have, instead of the animal/human figure, signs on the reverse as well, some of which correspond to the signs occurring on the faience and terracotta sealings, particularly the sign which, in copper tablets occurs as , , and .

Similarly, a comparative study of inscriptions from different sites is also required which will help in indicating changes in the script from region to region despite the high order of standardization attributed to the Harappans.

ACKNOWLEDGEMENTS

I am grateful to Shri I. Mahadevan for pointing out certain errors in my paper. Grateful thanks are also due to Shri K.N. Dikshit, Deputy Keeper, National Museum, and my colleague in the Survey, Shri Sital Banerjee, for showing me the material in the National Museum and the Antiquities Collection of the Archaeological Survey of India respectively. The drawings accompanying the paper are drawn by my colleagues Sarvashri A K. Ghose, M.S. Mani and S.S. Saar to whom I am beholden.

SL. No. | OBVERSE | REVERSE

1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31

FIGURE 35.1 *Inscribed copper tablets from Mohenjo daro.*

SL No	OBVERSE	REVERSE
32		
33		
34		
35		
36		
37		
38		

SL No	OBVERSE	REVERSE
39		
40		
41		
42		
43		
44		
45		

SL No	OBVERSE	REVERSE
46		
47		
48		
49		
50		
51		
52		

SL No	OBVERSE	REVERSE
53		
54		
55		
56		
57		
58		
59		
60		

FIGURE 35.2. *Inscribed copper tablets from Mohenjo daro.*

SL No	OBVERSE	REVERSE
61		
62		
63		
64		
65		
66		
67		
68		
69		
70		
71		
72		
73		
74		
75		
76		
77		
78		
79		
80		
81		
82		
83		
84		
85		
86		
87		
88		
89		
90	NO INSCRIPTION	
91		

FIGURE 35.3. *Inscribed copper tablets from Mohenjo daro.*

SL No	OBVERSE	REVERSE
92		
93		
94		
95		
96		
97		
98		
99		BULL
100		
101		
102		
103		BULL
104		
105		
106		
107		
108		
109		
110		
111		HARE
112		
113		
114		
115		
116		
117		
118		
119		
120		
121		
122		
123		

FIGURE 35.4 *Inscribed copper tablets from Mohenjo daro.*

FIND-SPOT AND OTHER AVAILABLE DETAILS OF THE COPPER TABLETS FROM MOHENJO-DARO

Serial No. (vide Figs. 35.1-35.4)	*Find Spot/Level*	*Published Reference/ Reg. No. and Size*
1	DK Area G Section Bl. 2, ho. II, rm. 24 LOWER LEVELS Level −5·03 m	Mackay, CIII, 7; p. 368 DK 4294 22·5 x 22·5 x 0·3 mm
2	DK Area G Section Bl. 1, ho. I, rm. 18 UPPER LEVELS Level −1·68 m	Mackay, XCIII, 13; p. 366
3	DK Area G Section Bl. 2, ho. IV, Court 15 UPPER LEVELS Level −3·23 m	Mackay, XCIII, 2; p. 364 DK 4346 33 x 13·3 x 1·25 mm
4	DK Area G Section Bl. 1 (Palace) Court III (2) UPPER LEVELS Level −3·08 m	Mackay, XCIII, 7; p. 365 DK 4532 37·5 x 20 x 2·5 mm
5	Not available	DK 3694 NM *
6	Not available	S 65 NM
7	Not available	DK 3931 SC, 63·10/254
8	DK Area G Section Bl. 6, ho. III, rm. 41 UPPER LEVELS Level −3·02 m	Mackay, XCIII, 8; p. 365 DK 4209 34·3 x 23·8 x 1·8 mm
9	Not available	DK 3532 SC, 63·10/286
10	Not available	DK 4209 Mackay, XCIII, 8
11	Not available	DK 10142 SC, 63·10/290
12	DK Area G Section Bl. 6, ho. II, rm. 10 UPPER LEVELS Level −3·05 m	Mackay, XCIII, 6; p. 365 DK 3692 22·5 x 22·5 x 3·3 mm

* NM = National Museum, New Delhi; SC, Central Antiquities Collection, Safdarjung, New Delhi

FIND-SPOT AND OTHER AVAILABLE DETAILS OF THE COPPER TABLETS FROM MOHENJO-DARO

Serial No. (vide Figs. 35.1-35.4)	*Find Spot/Level*	*Published Reference/ Reg. No. and Size*
13	DK Area G Section Bl. 6, ho. II, rm. 7 UPPER LEVELS Level −3·57 m	Mackay, XCIII, 5; p. 365 DK 4143 23 x 22 x 4 mm
14	DK Area G Section Bl. 6, ho. III, rm. 16 UPPER LEVELS Level −2·23 m	Mackay, XCIII, 12; p. 366 DK 3817 23 x 23 x 4·5 mm
15	Not available	DK 10695 SC, 63·10/265
16	Not available	DK 9015 SC, 63·10/265
17	DK Area G Section Bl. 1 (Palace) SW Wing (II) rm. 6 LOWER LEVELS Level −5·03 m	Mackay, CIII, 1; p. 367 DK 5444 35·3 x 25·4 x 1·8 mm
18	DK Area G Section Bl. 9, ho. VII, rm. 18 UPPER LEVELS Level −0·85 m	Mackay, XCIII, 9; p. 367, DK 3447 34·5 x 34·5 x 2·25 mm
19	Not available	DKi 715 SC, 63·10/248
20	Not available	DK 10146 SC, 63·10/288
21	Not available	DK 10175 SC, 63·10/272
22	Not available	DK 34 SC, 63·10/267
23	Not available	DKi 798 SC, 63·10/251
24	Not available	DK 12001 SC, 63·10/266
25	Not available	DK 10141 SC, 63·10/283
26	Not available	DK 1025/55 NM
27	Not available	DK 1347/58 NM
28	Not available	DK 1378 Hunter, III, 42 SC, 63·10/255

FIND-SPOT AND OTHER AVAILABLE DETAILS OF THE COPPER TABLETS FROM MOHENJO-DARO

Serial No. (vide Figs. 35.1-35.4)	*Find Spot/Level*	*Published Reference/ Reg. No. and Size*
29	DK Area G Section Bl. 6, ho. II, rm. 11 UPPER LEVELS Level −3·05 m	Mackay, XCIII, 3; p. 364 DK 3850 33 x 13·3 x 1·3 mm
30	Not available	DK 3447 SC, 63·10/247
31	DK Area G Section Bl. 10, ho. I, rm. 13 UPPER LEVELS −2·59 m	Mackay, XCIII, 14; p. 366 DK 4807 33·8 x 25·8 x 1·8 mm
32	Not available	DKi 489/60 NM
33	DK Area Trial Trench E −0·91 m	Marshall, Vol. II; pp. 399-400 E 2215
34	Not available	E 825 SC, 63·10/254
35	Not available	DK 1994 Hunter, III, 37
36	DK Area G Section Between Bls. I (Palace) and 2 (III), rm. 17 LOWER LEVELS −4·39 m	Mackay, CIII, 4; 367 DK 5421 36·3 x 13·8 x 3·3 mm
37	Not available	DK 552/76 NM
38	Not available	DK 11307 SC, 63·10/262
39	DK Area G Section Bl. 1, ho. VIII, rm. 63 LOWER LEVELS −5·73 m	Mackay, CIII, 3; p. 367 DK 5996 36·3 x 25·5 x 1·8 mm
40	Not available	DK 1838/56 NM
41	Not available	DKC 153/61 NM
42	Not available	B-103 SC, 63·10/304
43	Not available	DK 3169 SC, 63·10/309
44	Not available	E2. 1164 NM
45	Not available	DK 11146 SC, 63·10/246

FIND-SPOT AND OTHER AVAILABLE DETAILS OF THE COPPER TABLETS FROM MOHENJO-DARO

Serial No. (vide Figs. 35.1-35.4)	*Find Spot/Level*	*Published Reference/ Reg. No. and Size*
46	Not available	E 1092 SC, 63·10/303
47	Not available	DK 9152 SC, 63·10/265
48	Not available	E 814 SC, 63·10/279
49	Not available	C 2248 SC, 63·10/287
50	DK Area G Section Bl. 2 −1·83 m	Marshall, CXVII, 5; p. 399 DK 1606
51	Not available	DK 12514 SC, 63·10/278
52	Not available	DK 151 SC, 63·10/308
53	Not available	DK 3384 SC, 63·10/261
54	Not available	DK 7841 SC, 63·10/243
55	Not available	DK 5444 SC, 63·10/259
56	Not available	DK 10045 SC, 63·10/249
57	Not available	DK 10121 SC, 63·10/285
58	DK Area G Section Bl. 9, ho. VI, rm. 31 UPPER LEVELS −2·10 m	Mackay, XCIII, 10; p. 366 DK 3811 31·3 x 22·5 x 3·8 mm
59	Not available	DK 4225 SC, 63·10/246
60	Not available	DK 7874 SC, 63·10/274
61	DK Area G Section First Street (11) UPPER LEVELS Level −2·44 m	Mackay, XCIII, 4; p. 364 DK 3696 27 x 28 x 3 mm
62	Not available	DK 4927 NM
63	Not available	DK 7489 NM
64	Not available	DK 501/40 NM
65	DK Area G Section	Mackay, XCIII, 11; p. 366 DK 4672

FIND-SPOT AND OTHER AVAILABLE DETAILS OF THE COPPER TABLETS FROM MOHENJO-DARO

Serial No. (vide Figs. 35.1-35.4)	*Find Spot/Level*	*Published Reference/ Reg. No. and Size*
	Bl. 9, ho. VII, rm. 51 UPPER LEVELS Level −1·43 m	36·3 x 23·3 x 2 mm
66	DK Area G Section Bl. 2, ho. IV, Court 15 UPPER LEVELS Level −1·86 m	Mackay, XCIII, 1; p. 364 DK 3697 32·8 x 15·5 x 2·8 mm
67	Not available	DK 726 SC, 63·10/236
68	DK Area G Section First Street (21) LOWER LEVELS −4·51 m	Mackay, CIII, 5; p. 367 DK 8108 21·5 x 25 x 5·8 mm
69	DK Area G Section First Street (1) LOWER LEVELS −4·48 m	Mackay, CIII, 2; p. 367; DK 4235 36·3 x 24·3 x 24·3 mm
70	Not available	DK 3577/48 NM
71	Not available	DK 10596 SC, 63·10/306
72	DK Area G Section First Street (2) LOWER LEVELS Level −4·48 m	Mackay, CIII, 6; p. 367 DK 4157 28·5 x 22·5 x 22·5
Not included	DK Area G Section Bl. 1a, rm. 58 LOWER LEVELS Level − 5·88 m	Mackay, CIII, 12; p. 368 DK 5796
73	Not available	DK 10707 SC, 63·10/268
74	Not available	DK 4069 SC, 63·10/270
75	Not available	DK 4067 SC, 63·10/271
76	Not available	DK 5767 SC, 63·10/276
77	Not available	VS 702 Marshall, CXVII, 15
78	Not available	VS 192 Hunter, III, 24

Find-spot and other available details of the copper tablets from Mohenjo-daro

Serial No. (vide Figs. 35.1-35.4)	*Find Spot/Level*	*Published Reference/ Reg. No. and Size*
79	Not available	VS 3500 Marshall, CXVII, 7 Hunter, III, 30
80	Not available	VS 2590 Hunter, III, 31 SC, 63·10/282
81	VS Area House XVI rm. 107	VS 1988 Marshall, CXVII, 8; p. 400 Hunter, III, 29
82	Not available	VS 3320 Marshall, CXVIII, 4 Hunter, III, 27
83	Not available	VS 6058 SC, 63·10/242
84	VS Area House XXVI 0·304 m	VS 2028 Marshall, CXVII, 11; p. 400
85	Not available	VS 888 Hunter, III, 34
86	Not available	Hunter, III, 35
87	Not available	VS 2613 Marshall, CXVII, 1 Hunter, III, 32
88	Not available	VS 2360 Hunter, III, 39
89	Not available	VS 983 Marshal, CXVII, 3 Hunter, III, 40
90	Not available	VS 1406 Marshall, CXVII, 16 ARASI 1925-26, XLVI, 7
91	VS Area House 1, Rm. 30	VS 3526 Marshall, CXVIII, 5; p. 400
92	Not available	VS 2109 Marshall, CXVIII, 6 Hunter, III, 45
93	Not available	VS 2432 Hunter, IV, 54
94	Not available	VS 3590 Marshall, CXII, 2
95	Not available	VS 1104 Hunter, III, 52 SC, 63·10/241

FIND-SPOT AND OTHER AVAILABLE DETAILS OF THE COPPER TABLETS FROM MOHENJO-DARO

Serial No. (vide Figs. 35.1-35.4)	*Find Spot/Level*	*Published Reference/ Reg. No. and Size*
96	Not available	VS 1959 SC, 63·10/277
97	HR Area Bl. 3, House XVII Court 39	Marshall, CXVII, 12; p. 400
98	Not available	HR 5549 SC, 63·10/237
99	Not available	HR 723 Hunter, III, 25
100	Not available	HR 2743 Marshall, CXVIII, 1 Hunter, III, 23
101	HR Area Bl. 5, Street XXXIX, 0·91 m	HR 4573 Marshall, CXVII, 14; p. 400
102	Not available	HR 5816 Hunter, III, 50
103	Not available	HR 2882 Hunter, III, 51
104	Not available	HR 115 Hunter, IV, 62
105	Not available	HR 4337 Marshall, CXVIII, 3 Hunter, III, 48
106	Not available	HR 2676 Hunter, III, 47
107	Not available	HR 2984 Marshall, CXVII, 10 Hunter, IV, 55
108	Not available	HR 4799 Hunter, IV, 58
109	Not available	HR 4615 Hunter, IV, 57
110	L Area Bl. 4 Pillared Hall	Marshall, CXVII, 6; p. 400 L 456
111	Not available	L 459 Hunter, III, 44
112	L Area Bl. 4 Chamber 27	L 461 Marshall, CXVII, 4; p. 400
113	L Area Block 4 Chamber 22	L 982 Marshall, CXVII, 13; p. 400
114	Not available	SD 1517 Marshall, CXVII, 2 Hunter, III, 38

FIND-SPOT AND OTHER AVAILABLE DETAILS OF THE COPPER TABLETS FROM MOHENJO-DARO

Serial No. (vide Figs. 35.1-35.4)	*Find Spot/Level*	*Published Reference/ Reg. No. and Size*
115	Not available	SD 3009 SC, 63·10/239
116	Not available	SD 3032 SC, 63·10/269
117	Not available	SD 2554 Hunter, III, 36
118	Not available	SD 2051 Hunter, III, 49
119	Not available	SD 1758 Hunter, III, 46 SC, 63·10/307
120	Not available	SD 1200 Hunter, IV, 61 SC, 63·10/238
121	Not available	SC, 63·10/280
122	Not available	SC, 63·10/256
123	Not available	SC, 63·10/298

FIGURE 35.5. *Inscribed copper tablets from Mohenjo daro incidence of end signs.*

AREA	DK AREA					VS AREA			HR AREA					L AREA			SD AREA			AREA NOT KNOWN
CLASS	A	B	B1	C	H	A	B	NO. INSC.	A	B	C	E	G	A	B	D	A	B	F	A
INSCRI-PTIONS ENDING IN																				
	45 (+1)[1] (+1)[2] (+1)[3] (+2)[4]	19 (+2)[1] (+1)[3] (+1)[4]	1	5 (+1)[3]	(1)[2]	13	5	1	4	6	1	1	1	1 (+1)[2]	1	2	4 (+1)[2]	2	1	2
TOTAL	50	23	1	6	1	13	5	1	4	6	1	1	1	2	1	2	5	2	1	2

INSCRIPTION										
ANIMAL HUMAN FIGURE SYMBOL										
SL. NOS. (AS PER TABLE-I	4-7	14,15,87, 100	43-46, 84	17-27, 81-83 97-99, 115, 122	39-42	29-34	10, 11, 89	58-60	50-55, 92, 119	70-73
INSCRIPTION										
SYMBOL ANIMAL FIGURE							AND VARIANTS			
SL. NOS.	12, 13	16, 79	2	8, 9	3	61-63, 91	47-49		94	64-67

FIGURE 35.6. *Inscribed copper tablets from Mohenjo daro legends vis-a-vis animal and human devices.*

Part IX The Post-urban Phase of the Indus Civilization

Editor's Introduction

It is certain that the two great cities of the Indus Civilization, Mohenjo daro and Harappa, were completely abandoned by approximately 1900 B.C. That this eclipse of the centers took place over many years, possibly a century or two, is best documented from Mohenjo daro where the Late level architecture is poorly constructed and there is extensive utilization of used and broken bricks. In addition, buildings encroach onto streets and other "public space" and noxious facilities such as pottery kilns are used within civic bounds. All of this can be said to indicate a deterioration in the codes and norms of behavior which in the earlier periods of this settlement seem to have been more perfectly intact. Early in the second millennium B.C. the city as an institution, as a focal point of civilized life in Sind, was apparently in the grip of profound change which eventually led to its complete demise. Clearly the process by which this took place is of utmost importance to both the cultural history of the civilization and an understanding of the organizational and operational processes which characterized it.

There has been much speculation on the eclipse of the cities. Surprisingly, Sir John Marshall has little to say on the matter; however, E.J.H. Mackay suggested that the roots of the process were more likely to be found in the vagaries of the Indus River than with invaders or other cultural factors (Mackay 1938b:6). Wheeler, on the other hand, suggested in his 1946 report of excavations at Harappa (reprinted here) that it was likely to have been the invading Aryans who destroyed the Indus settlements and the Harappan cultural tradition.

Many problems emerge in a serious entertainment of this "Aryan Hypothesis." First, the chronology of the Indus Civilization, at least the Urban Phase during which the cities were fully occupied, cannot be projected to have a terminal date beyond 1800 B.C. It is widely agreed that the Vedic literature from which the hypothesis is drawn was compiled sometime between 1200 and 800 B.C. This leaves us with a chronological gap of between 600 and 1000 years, and it hardly seems reasonable to postulate that this discrepancy can be filled by the period between the historical destruction of the cities and the compilation of the Vedas. Of crucial importance as well, is the fact that there are no signs of a military assault on either of the cities or other excavated sites. Wheeler refers to a massacre at Mohenjo daro (1947a.82) but even this has been doubted by Dales in a paper selected for this part.

It has been argued (Possehl 1974:21-42) that the Indus Civilization did not "end" with the abandonment of the cities and some of the other settlements with Urban Phase components since there is a sound archaeological basis for a Post-urban Phase to the Indus cultural tradition in Sind, Gujarat and the Punjab. Such changes as are evident in the archaeological record do not necessarily suggest an invasion as much as a normal process of cultural modification which characterizes all human life. We need not look to some external force to explain this case. The causes are to be found within the Harappan cultural tradition itself.

What, more specifically, might be used to explain the abandonment of the cities? It could be argued that we must look directly into the structure of Harappan *urban* life for some flaw. In other words, if the cities were abandoned then it must have been caused by something characteristically urban. If this is accepted then a range of explanations for the "end" of the Urban Phase would be sought in those elements of society and culture which find themselves most clearly expressed

there. This brings to mind institutions such as kingship, and the differentiated nature of city-based political institutions; economic factors such as craft and productive specialization including the relationships necessary to unite them with a larger organic whole; systems of trade and tribute which are necessary for urban elites and their particular style of life; and the differentiated social structure which has been presumed to characterize urbanization. Other factors with a particular urban base might also be considered. For example, diseases such as typhoid or cholera which require host populations of significant size. This is certainly not an exhaustive enumeration of what might be sought if it were decided that the eventual solution to the abandonment of the Indus cities must be found within the fabric of the city itself. It will not be enlarged here because there is another alternative: that the abandonment of the cities had little or nothing to do with their organization *per se*.

There is a view of civilization which suggests that all settlements within an urban system are dependent upon a much broader range of products, services and relationships than those at other orders of sociocultural development. If this is accepted, if one stresses the dependent nature of one part of a civilization on all, or most, of the others then changes in structures far removed from those characteristic of the city could have immense consequences there. A kind of ancient "domino effect" could ensue which would seal the fate of the cities as surely as any of the factors enumerated above. In this case a range of explanatory devices comes to mind which is significantly different from those noted above. For example, floods, crop failure, climatic change and an almost unlimited number of other natural phenomena could be invoked. Political conflict which could destroy the people producing the food and/or the productive potential of the land on which the non-agricultural urbanites ultimately depended could be another such explanation. Might we not even entertain such notions as peasant revolts against the ruling classes who may have been largely, or even exclusively city based? Changes in peasant social order, or in the cooperative relationships between villages, have an explanatory potential which need not include characteristically urban phenomena. Again, a longer "check list" of potential devices which might be isolated and eventually tested could be enumerated. But this would miss an important point.

A satisfactory explanation for the abandonment of the Indus cities cannot be presumed to be simple, or amenable to bold historical insight. If we are to be satisfied, a solution to the problem must be one which has the full structure of an argument and a relationship to culture historical fact. At this moment the only culture-historical facts that we have to explain are: (1) the cities were abandoned; (2) at Mohenjo daro and probably at Harappa this took place over a century or two; (3) Post-urban settlements in Sind, Gujarat and the Punjab do not indicate a general population decline. These three facts can obviously be adequately encompassed by a large number of "theories" and without further culture-historical facts, which are after all what we are attempting to explain, our arguments will remain thin and vulnerable.

One of the major theories on the end of the Indus cities has been stated by Robert Raikes and supported by George Dales. Two of their papers are reprinted here. Their theory incorporates the Indus floods and would fall within the range of "domino theories" noted above. H.T. Lambrick's paper is a critical assessment of the Raikes and Dales position.

The final paper of this part, by H.D. Sankalia, reviews some specific aspects of the so-called "Cemetery H Culture" which is one of the Post-urban Phase assemblages of the Punjab.

Harappan Chronology and the Rig Veda

SIR MORTIMER WHEELER

The fixed point in the chronology of the Harappa culture is that, in a characteristic phase, it was in contact with Sumer in and about the time of Sargon of Agade (Akkad), now dated to *c.* 2350 B.C. (Gadd 1932; Piggott 1943, 1946).[1] The evidence consists primarily, though by no means exclusively, of seven (or possibly eight) seals of Indian origin or type found at Ur (1 or 2), Kish (2), Tell Asmar (2), Tepe Gawra (1) and Susa (1) in associations certainly or probably of Sargonid date. Unfortunately no type-sequence of the seals of the Harappa civilization has yet been detected, and in any case four at least of the seven in question are of types which appear to occur at all the excavated levels of Mohenjo-daro. Of the other three, one from Tell Asmar is a cylinder which, though clearly of Indian workmanship, is not closely comparable with any of the three cylinder-seals found actually at Mohenjo-daro; and the fact that the latter were recovered respectively at 5.9 feet, 11.8 feet and 14.5 feet below datum means—if it means anything at all—that the cylinder was there almost equally long-lived. The square seals incised with concentric squares from Tell Asmar and Tepe Gawra are comparable only with a rare Mohenjo-daro type of which little is known. In other words, the Akkadian "fixed point" is a very mobile one in so far as the internal chronology of the Harappan civilization is concerned. Its significance is limited to the indication—an important one, so far as it goes—that in the period of Sargon the Harappans were in livelier contact with the West than at any other time.[2]

There is indeed other seal-evidence which might be expected to bracket the Sargonid series and so add a little definition to the picture. Two Indo-Sumerian seals have been ascribed to the pre-Akkadian period, both from Ur. One, only vaguely Indian, is dated as pre-Akkadian by reason of the archaic cuneiform inscription which it bears; the other was found in the filling of a tomb-shaft ascribed by Woolley to the elusive Second Dynasty of Ur but by Frankfort to the Akkadian period (Frankfort 1939:306). Its nearest analogy (Mackay 1938b: pl. 96, no. 500) was found at Mohenjo-daro at a depth of 14.8 feet below datum and presumably therefore occupies an intermediate position in the excavated series. But altogether the "pre-Akkadian" evidence does not effectively extend our chronology backwards.

At the other end of the bracket, three Indian seals seem to occur in post-Akkadian contexts at Ur (2) and Lagash (1). A crude cylinder-seal from Ur was found in a Larsa tomb which may be dated to the beginning of the second millennium B.C. That the crudity of the workmanship is not in itself evidence of relative date is indicated by the apparently undifferentiated occurrence of good and bad seals at all excavated levels of Mohenjo-daro. The seal from Lagash is said to belong to the same period (de Genouillac 1930:177). Yet another from Ur occurred in "upper rubbish, Kassite (?) level," which is not satisfactory stratification but *may* indicate a date as late as the sixteenth or fifteenth century B.C. Incidentally, the type is remarkable: it represents a man carrying a yoke wherefrom hang objects which have been interpreted as water-skins or pots. They are more probably fishing-nets, each containing a fish. A similar theme—a man between two nets with star-like objects in the background—is represented on a potsherd from Harappa (Vats 1940: pl. 69, no. 16).

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The seals as a whole, therefore, carry us from the eve of the Akkadian period to the beginning of the second millennium, with possible though doubtful intrusion into the middle of that millennium. How far is this indication amplified from other sources?

First, the copper pins. At Chanhu-daro in Sind, Mackay found a double-spiral copper pin allegedly in the Harappa levels. But in spite of the distinctive character of this well-known type, it offers no present aid to our problem. A type which occurs on the one hand in Sialk IV (probably the latter part of the fourth millennium) and on the other hand in the *terramare* of Italy two thousand years later has no chronological significance in this context, until local values are determined independently. A single-spiral copper pin found at Mohenjo-daro at a depth of 18.4 feet below datum and therefore presumably derived from a fairly early phase of the occupation is also a widespread type to which it would be equally perilous to attach importance; and the occurrence of several roll-top pins of Hissar IIIB forms in the Jhukar occupation which overlies the Harappa levels at Chanhu-daro has a derivative rather than a contributory interest. The pins in fact do not appreciably help.

Secondly, a copper axe-adze found 6 feet below the surface of Mohenjo-daro is paralleled in Early Minoan II, Troy II, and Hissar III, and lasted into the second millennium B.C. in Europe and perhaps the Caucasus. This therefore adds nothing to the seal-dating.

Thirdly, a more definitive discovery is that of an unpublished bronze or copper knife of distinctively curved Harappan type (Vats 1940:pl. 122, no. 6)[3] found by Dr. Erich Schmidt at Hissar in stratum IIIB. The dating of Hissar III is under discussion; McCown's chronology equates Hissar IIIB with Early Dynastic II-III of Ur, but Piggott would make it overlap the Akkadian period.

Fourthly, there are a circular steatite pyxis or box (a fragment probably of the Mesopotamian "hut" type) found at Mohenjo-daro by Mackay at the considerable depth of 28.1 feet below datum, "in a very early stratum," and the two rectangular stone boxes found previously by Marshall at depths of only five and seven feet (Mackay 1938b:321, pl. 142, no. 45 and Marshall 1931:369, pl. 131, no. 37). Similar vessels occur at Ur, Kish, Khafajah and Susa (McCown's D) (Mackay 1932:357), and, nearer India, on the undated but perhaps late Harappan site of Mehī in southern Baluchistan and elsewhere.[4] In Mesopotamia these boxes seem to be characteristic of Early Dynastic III, but in north-eastern Iran simple examples such as those from the higher levels of Mohenjo-daro occur in Shah Tepe IIA and Hissar IIIC (McCown 1942: Fig. 17), which are Early Dynastic III—Akkadian on McCown's chronology or late Akkadian (*c.* 2000 B.C.) on Piggott's. Indeed the deep-level "hut" box from Mohenjo-daro is at present almost the *only* piece of definitive evidence from the Harappan civilization that need go back beyond Sargon.[5]

The more general relationships, direct or indirect, between the Harappan culture and the other Indo-Iranian cultures of the chalcolithic phase do not at present help appreciably to narrow or confirm the absolute dating of the former. They constitute an alluring and important study, and they are engaging the attention of an increasing number of very competent scholars. The first results show abundantly that in these comparative studies the time-factor must not be pressed too hard. The cultures in question were liable to an uncanny durability, particularly in phases or areas of economic equilibrium, and there is ample scope for differential development of a baffling kind from region to region. Mention has been made above of the wide extension of certain metal types in time no less than in space. In pottery a similar, though more surprising, persistence cannot be discounted. Marshall's first impression that there was "but little difference in style and technique at the various levels" of Mohenjo-daro is modified but not contradicted by Mackay's later results. Mackay's plates show an apparent persistence of distinctive types and techniques (including even the elaborate polychrome decoration) from first to last, associated from time to time perhaps with other types of alien or relatively transitory character. These results need checking by more exact methods of excavation. Nevertheless in 1946 the careful digging of a small area on the platform of the Harappa citadel (section HP XXX), with this problem in mind, showed the undoubted continuance of the mature Harappa culture through the six successive building-phases of the site. True, the same excavation revealed a variant culture at a lower level, beneath the defences; but the six phases of substantial baked brick construction of the upper levels may be regarded as the product of several centuries, perhaps four or five in number.[6] On the same calculation, the ten occupation-levels of Mohenjo-daro might, so far as excavated, represent more than seven centuries of essentially uniform ceramic. And we must remember that at Mohenjo-daro the underlying natural surface

has never been reached, whilst at Harappa the largest mound other than the citadel—Mound E—has not even been trenched. The duration of the Harappa culture in terms of building-construction may well be even greater than can at present be calculated.

But, by way of giving verisimilitude to this astonishing stagnation, certain elements of change are in fact identifiable, and others doubtless await discovery. Thus Mackay notes that, at Mohenjo-daro, handmade ware "is uncommon in the upper levels, but we have a good many examples from the lower levels." A similar differentiation applies to the incised ware from the site. The remarkable glazed ware, of light grey fabric covered with a polished purplish slip which was then glazed and combed with straight or wavy lines, comes only from very early levels. These and other minutiae are at present too isolated to tempt further research. On the other hand at Harappa, apart from the seeming restriction of a class of small seals to the lower levels (Vats 1940:324), there are in fact two significant differentiae which are likely to develop in importance. The first of these is the occurrence, mentioned above, of a series of potsherds of non-Harappan type in a stratum heavily sealed by the citadel-defences. Whether these sherds represent a proto-Harappan culture or, more probably, an alien village-culture such as that of Periāno-Ghuṇḍai is not certainly deducible from the relatively small amount of material available, but the problem is one which must be watched in future excavation. The second of the differentiae relates to the other end of the story; it is the Cemetery H industry (two phases but apparently interrelated), which is now seen to be superimposed upon the Harappā culture after the deposition of a considerable mass of intervening débris. The intrusive culture, as represented by its pottery, has in origin nothing to do with the Harappa culture; its ceramic differs from that of the latter both in finish and in decoration, and its dwellings, as identified for the first time in 1946 on the Western Terraces of the citadel, are notably more roughly constructed than those of Harappa proper. Its analogues have not yet been identified, and it appears in fact as abruptly as did its Harappan predecessor. The suggestion has indeed been made, very hesitantly, that the Cemetery H intruders "may belong to the Aryan invaders" (Childe 1952:223)[7] the conventional date for whose first incursion into India is the fifteenth century B.C. And here the risk which Indian archaeology is always ready to run in the search for a literary context lies once more across our path.

Nor am I altogether disinclined to face that risk. The Aryan invasion of the Land of the Seven Rivers, the Punjab and its environs, constantly assumes the form of an onslaught upon the walled cities of the aborigines. For these cities the term used in the Rigveda is *pur*, meaning a "rampart," "fort" or "stronghold." One is called "broad" (*pṛithvī*) and "wide" (*urvī*). Sometimes strongholds are referred to metaphorically as "of metal" (*āyasī*).[8] "Autumnal" (*śāradī*) forts are also named: "this may refer to the forts in that season being occupied against Aryan attacks or against inundations caused by overflowing rivers." Forts "with a hundred walls" (*śatabhuji*) are mentioned. The citadel may be made of stone (*aśmamayī*): alternatively, the use of mud-bricks is perhaps alluded to by the epithet *āmā* ("raw," "unbaked"). Indra, the Aryan war-god, is *puraṁdara*, "fort-destroyer." He shatters "ninety forts" for his Aryan protégé, Divodāsa. The same forts are doubtless referred to where in other hymns he demolishes variously ninety-nine and a hundred "ancient castles" of the aboriginal leader Śambara. In brief, he "rends forts as age consumes a garment."

Where are—or were—these citadels? It has in the past been supposed that they were mythical, or were "merely places of refuge against attack, ramparts of hardened earth with palisades and a ditch." The recent excavation of Harappa may be thought to have changed the picture. Here we have a highly evolved civilization of essentially non-Aryan type, now known to have employed massive fortifications, and known also to have dominated the river-system of north-western India at a time not distant from the likely period of the earlier Aryan invasions of that region. What destroyed this firmly-settled civilization? Climatic, economic, political deterioration may have weakened it, but its ultimate extinction is more likely to have been completed by deliberate and large-scale destruction. It may be no mere chance that at a late period of Mohenjo-daro men, women and children appear to have been massacred there (Mackay 1938b:94f., 116ff., 172). On circumstantial evidence, Indra stands accused.

The combined weight, such as it is, of these various indications suggests the millennium 2500-1500 B.C. as a possible inclusive date for the mature Harappa civilization, without prejudice to the still-unplumbed depths of Mohenjo-daro.[9] But in conclusion let it be

squarely stated once more that the Akkadian contacts are the only well-fixed points. Material for objective dating in the post-Akkadian period is at present very slight. The relative abundance of Harappan objects on Akkadian sites may be taken to imply that, in and about the time of Sargon, Harappan enterprise—presumably commercial[10]—reached its apogee. This in turn may, in the normal order of things, be taken to imply bracketing phases of rise and decline, extending on the one hand into the Early Dynastic period and on the other hand into the second millennium. I have therefore invoked Indra; nevertheless, even Indra's hostile citadels may be represented, not by the Harappan sites, but by others yet unknown to us. If so, we have to assume that, in that short interval which can, at the most, have intervened between the end of Harappa and the first Aryan invasions, an unidentified but formidable civilization arose in the same region and presented an extensive fortified front to the invaders. The assumption is not an easy one, and seems to involve a wilful rejection of the massive fortifications with which the Harappans are now known to have girt themselves. Digging, and more digging, will ultimately solve the problem.

NOTES

[1]The shortened dating of Hammurabi (1792-1750) suggested by Sidney Smith (1940) is vital to the argument which follows, and, in the deficiency of the necessary apparatus in India, I am indebted to Professor Gordon Childe for correspondence regarding the application of this dating to the earlier periods of Mesopotamian chronology.

[2]The evidence of the seals is reinforced by potsheads, etched beads and kidney-shaped inlays of bone, all of Harappan types, found in Akkadian houses at Tell Asmar (See Frankfort 1933:48ff.). Gold and faience disc-beads with axial tube occur at Mohenjo daro and Harappa (Marshall 1931:522-3, pl. 146, no. 34 and pl. 149, no. 7; Vats 1940: pl. 133, no. 3), in Summer in Early Dynastic III—Akkadian contexts, and, consistently, in Troy II. The gold examples from Mohenjo daro were found at a depth of 6 feet with scrap metal, which suggested a "goldsmith's hoard of metal for melting." It is uncertain, therefore, how old they were at the time of burial. For the type, see McCown (1942:53, and table I) and Childe (1952:185, 195, 213). Editor's note: the Childe citation in this footnote was to the 1934 edition of Childe (1952), *New Light on the Most Ancient East.*

[3]I owe the information as to the Hissar example to the kindness of Dr. Donald McCown.

[4]Mehi has produced one rectangular and three circular examples in stone and two circular pottery-imitations, all in the Central Asian Antiquities Museum, New Delhi. For other sites, see Piggott (1943:176).

[5]Dr. McCown, in conversation, is inclined to ascribe the oblique cruciform pattern on one of the shell-plaques of the game-board from the royal tomb PG 789 at Ur to Harappan influence, comparing the cruciform pattern on a silver ring from Mohenjo daro. This comparison might add a second contact with Early Dynastic III. See Woolley (1934:277, pl. 96), Marshall (1931:520, pl. 148, no. 13) which is better illustrated in Mackay (1931g: pl. facing 459, no. 5).

[6]The salt which today rapidly disintegrates baked brick on exposure both at Harappa and Mohenjo daro would be considerably less abundant and noxious if the soil were regularly cultivated and the surface water which now evaporated through the desert sand and drags up with it the deep living salt were absorbed systematically by plant life. There is no evidence that anciently the walls of these cities suffered materially from salt. It seems reasonable therefore to assume something like seven or eight decades as the lifetime of a Harappan building well constructed of baked bricks. Mackay (1938b:47-8) is wrong in his inferences from modern salt action.

[7]The original reference was to the 1934 edition of *New Light*. . . .

[8]The exact meaning of *ayas* in the Rig Veda is uncertain. If it does not merely imply "metal" generically, it may refer rather to bronze than to iron. See Macdonell and Keith (1912:I, 31).

[9]It must not, of course, be assumed that the unexplored lowest levels of Mohenjo daro are necessarily "Harappan," any more than is the lowest level at Harappa itself.

[10]If so, the extreme rarity of Mesopotamian objects at Harappa and Mohenjo daro implies that the trade was balanced in consumable goods. But what?

The Mythical Massacre at Mohenjo Daro

GEORGE F. DALES

Nothing delights the archaeologist more than excavating the ruins from some ancient disaster—be it a flood, earthquake, invasion, or massacre. This does not reflect any inordinately ghoulish tendency in the character of archaeologists. It is simply that a much more complete picture of the life and times of an ancient site is preserved if it was the victim of some quick, devastating disaster than if it had just died a slow natural death, had been abandoned or remodeled.

The classic example of the rewards that we can reap as the result of an ancient natural disaster is Pompeii where the eruption of Vesuvius preserved for posterity a full-scale authentic model of daily life in an ancient Roman town. But more popular with historians are disasters that can be blamed on mankind itself. Scholars and laymen alike have always delighted in being able to boo and hiss the evil villain, the murderous invader, the barbarian hordes. Only the approach is different—the one flicks on the "Late Show," the other writes learned footnotes.

One of the most enigmatic whodunits of antiquity concerns the decline and fall of the Indus Valley (Harappan) civilization. Remains of this vast civilization of South Asia are scattered over an area considerably larger than those covered by either ancient Egypt or Mesopotamia. The life cycle of this third major experiment in the origin and development of the world's earliest civilizations is at present highly speculative and is the subject of increasingly intensive investigation by archaeologists, historians, linguists, and natural scientists alike.

It is now apparent that a re-evaluation is necessary of some of the earlier theories that have come to form over the past thirty years the basic structural members in the framework of early South Asian history. It is especially necessary to call for a retrial concerning the placing of guilt for the demise of the Indus civilization. Evidence was published some 30 years ago suggesting that Mohenjo-daro, the southernmost of the two major cities of the Harappans, was destroyed by armed invaders and that the hapless victims—including a large percentage of women and children—were massacred on the spot. The excavators of Mohenjo-daro were content—at least at first—to put the blame for the "massacres" on several disassociated causes and incidents. The "massacre" idea immediately ignited and has been used as a torch up to the present day by some historians, linguists, and archaeologists as visible, awful proof of the invasion of the subcontinent by the Aryans. It provided a seemingly pat answer to one of the most vexing questions in South Asian history. The arrival into northern India of the Aryans—the eastern branch of the vast Indo-European language family—heralded the beginning of the historical era in South Asia. The social and religious life of the times is described in detail in the hymns of the Sanskrit *Rig-Veda*, the earliest book known in India. The Vedic hymns describe the principal god, Indra, as the "fort destroyer" who "rends forts as age consumes a garment." In attacking the fortresses of the *dasyu* (the name applied to the non-Aryan enemies, be they mortal or supernatural), Indra is specifically described as setting fire to the buildings:

> *. . . in the kindled fire he burnt up all their weapons, and made him rich with kine and carts and horses.*

The texts describe how the Aryan warriors were protected by armor and shields. In addition to the bow

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and arrow—the chief weapon—they used the javelin, axe, and sword. Horses were common but were probably used to pull the chariots rather than for riding.

It seems logical to assume that, as Sir Mortimer Wheeler put it, "Indra stands accused" of destroying the cities of the Harappan civilization and of the responsibility for the "massacre" at Mohenjo-daro. Apart from a few dissenting comments in rather obscure publications, the general literature on the subject current today still repeats vivid, dramatic descriptions of the barbarian hordes descending upon the once great and proud cities of the Indus civilization. For example:

> *The Indus cities fell to barbarians who triumphed not only through greater military prowess, but also because they were equipped with better weapons, and had learnt to make full use of the swift and terror-striking beasts of the steppes* (i.e. the horse).
>
> (Basham 1954:27)

It is still premature to talk in terms of absolute dates—the entire chronology of South Asia down to the 6th century B.C. is a web of pluses and minuses of hundreds of years—so, on purely chronological grounds, we cannot even establish a definite correlation between the end of the Indus civilization and the Aryan invasion. But even if we could, what is the material evidence to substantiate the supposed invasion and massacre? Where are the burned fortresses, the arrowheads, weapons, pieces of armor, the smashed chariots and bodies of the invaders and defenders? Despite the extensive excavations at the largest Harappan sites, there is not a single bit of evidence that can be brought forth as unconditional proof of an armed conquest and destruction on the supposed scale of the Aryan invasion. It is interesting that Sir John Marshall himself, the Director of the Mohenjo-daro excavations that first revealed the "massacre" remains, separated the end of the Indus civilization from the time of the Aryan invasion by two centuries. He attributed the slayings to bandits from the hills west of the Indus, who carried out sporadic raids on an already tired, decaying, and defenseless civilization.

What of these skeletal remains that have taken on such undeserved importance? Nine years of extensive excavations at Mohenjo-daro (1922-31)—a city about three miles in circuit—yielded the total of some 37 skeletons, or parts thereof, that can be attributed with some certainty to the period of the Indus civilization. Some of these were found in contorted positions and groupings that suggest anything but orderly burials. Many are either disarticulated or incomplete. They were all found in the area of the Lower Town—probably the residential district. Not a single body was found within the area of the fortified citadel where one could reasonably expect the final defense of this thriving capital city to have been made.

It would be foolish to assert that the scattered skeletal remains represent an orderly state of affairs. But since there is no conclusive proof that they all even belong to the same period of time, they cannot justifiably be used as proof of a single tragedy. Part of this uncertainty results from the unsatisfactory methods used by the excavators to record and publish their finds. But even allowing for this serious methodological shortcoming, it is possible to re-evaluate the published evidence and to come to some definite conclusions concerning the massacre myth.

The most celebrated group of skeletons, the photograph of which is usually published to provide visible proof of the "massacre," was found in the area of Room 74, House V (HR area) (see Marshall 1931: pl. XLIV, a, and pl. XLVI, a). The interpretation of this grisly discovery was not even agreed upon by the excavators themselves. Hargreaves, who did the actual excavating, states that because four of the fourteen skeletons were found above the ruins of the southern wall of the room, the entire group belongs to a date subsequent to the decay of the building and thus to a period posterior to the abandonment of the latest stage of the city. Marshall, the over-all director of the excavations, says on the other hand "this does not seem to be proven." He points out that the building belongs to the Intermediate period of the city and that this entire area was covered over and rebuilt in the Late period (the assumed Late period remains were not preserved at this part of the city; it is probable they had eroded away). Marshall suggests that the skeletons could belong to the interval between the Intermediate and Late periods, "though the possibility of their being posterior to the Late period may be admitted." He also disagrees with Hargreaves over the circumstances that produced this gruesome spectacle. Hargreaves stated with questionable perspicacity that the fourteen bodies "appear to indicate some tragedy." Furthermore, he observed that the twisted, intermingled positions of the bodies are those "likely to be assumed in the agony of death than those of a number of corpses thrown into a

room." Marshall read the evidence differently. He believed that the bodies were intentionally interred "within a few hours of death" or else they would have been prey for animals and birds. "There is no reason whatever for doubting that these burials date from the declining years of Mohenjo-daro's prosperity," stated Marshall, but he didn't suggest they represent any final massacre of the population.

During the removal of the thick accumulation of debris covering a courtyard of the Intermediate period (House III, HR area), incomplete remains of three skeletons were found. Their location in the debris shows, however, that they did not belong to the time of the courtyard but to sometime after it had fallen into disuse and had been filled in, possibly in preparation for the buildings of the Late period. The excavator suggests that it represents a late funerary deposit and doesn't intimate any connection with a final "massacre" of the city's population. Those who have so stated have misread the archaeological evidence.

One reads about "the slaughtered Harappans" who "lay unburied amid their streets." This melodramatic description was prompted, in part, by the reported find of six skeletons in a lane between two houses in the VS area of Mohenjo-daro. And yet, the excavator stated in his report that "from their position they appear to be posterior to the adjacent remains." They were covered with loose earth, free from bricks and other debris that would indicate any violent destruction. There is no suggestion in the report that they were lying on the actual street surface. Marshall suggests again that they were probably burials of the Late period that just accidentally penetrated down between the building walls bordering the lane—the lane itself having been long before covered over. Had the skeletons really been found directly on the street surface, there would still be no case for a final "massacre" because the lane belongs to the Intermediate period of the city.

Deadman's Lane in the HR area of the city was the scene of another well publicized but mythical street slaughter. One fragmentary skeleton (part of a skull, the bones of the thorax, and the upper arm of an adult) was found lying on its back diagonally across the narrow lane. But this incomplete skeleton was not resting directly on the walking surface of the lane. It appears to have been in the debris that accumulated between the walls of the building facing the lane sometime after the lane had fallen into disuse. The lane itself belongs to the Intermediate period of the city. This area was rebuilt during the Late period and houses covered the location of the earlier lane. The excavator suggests that this partial skeleton was interred under the floor of a house of the Late period. Thus, it was just accidentally located in the proximity of the lane and was not associated with it at all.

Another celebrated group of "victims" consists of nine skeletons that "lay in strangely contorted attitudes and crowded together" (Block 10A, DK area). Ernest Mackay, the excavator, expressed considerable doubt about the date of these remains. They were reportedly found at a level corresponding to the early part of the Intermediate period. For "convenience sake," Mackay termed the find-spot a burial pit although he admitted that he noticed no definite walls for the "pit" nor any traces showing that the area had been dug. Only two objects were found with the skeletons—an ivory comb that is not like the known Harappan period combs, and a copper bracelet. On the evidence of the bracelet, Mackay dates the remains to "the period of the occupation of the city." The technical report on the skeletal remains states that they probably do not represent a massacre *per se* because many of the skeletons were incomplete, represented by only a few fragments of cranium and odd bits of bone.

Mackay suggests that these were the remains of a family who tried to escape from the city with their belongings at the time of a raid but were stopped and slaughtered by the raiders. Their bodies were then "thrown pell-mell into a hurriedly made pit." He says it is "quite possible" that the tragedy took place in the final period of the city but can offer no supporting evidence. That at least five of the nine skeletons were of children prompted the anthropologist who studied the remains to conclude that "the raiders nursed a consistent hatred of the people of Mohenjo-daro as a whole, and total extermination appears to have been their endeavour."

Finally, in bringing this rather macabre account to an end, mention must be made of the lone bit of evidence from Mohenjo-daro that could conceivably be used as positive evidence of some murderous tragedy during the Late period of the city. In what we might call the "Well Room Tragedy" (DK area, G section) (see Mackay 1938b: pl. XLIII, c), two skeletons were found on a flight of stairs "evidently lying where they died in a vain endeavour with their last remaining strength to climb the stairs to the street." But the circumstances surrounding this tragedy are unknown and it would be presumptuous to cry "massacre" on this bit of evidence alone.

Thus stands the evidence in the case against Indra and the Aryans, or to be less specific, against the idea of a "final massacre" by whomever you prefer. The contemporaneity of the skeletal remains is anything but certain. Whereas a couple of them definitely seem to represent a slaughter, *in situ*, the bulk of the bones were found in contexts suggesting burials of the sloppiest and most irreverent nature. There is no destruction level covering the latest period of the city, no sign of extensive burning, no bodies of warriors clad in armor and surrounded by the weapons of war. The citadel, the only fortified part of the city, yielded no evidence of a final defence.

The evidence that is being gathered by present investigators from various branches of the natural and physical sciences is tending to support—in part—the theory expressed years ago by Mackay. Regarding the decay of Mohenjo-daro and the Harappan civilization, he suspected the cause to be "the vagaries of the Indus rather than pressure by invaders, of whose existence we have, in fact, little positive evidence."

The details of the story of the decline and fall of the Indus civilization are, as yet, far from clear, but a pattern of contributing factors is taking shape. This pattern does not include invasion and massacre as basic factors. On the contrary, it appears that a series of natural disasters occurred—possibly as swiftly, certainly more devastating than any hypothetical invasion. A sudden rise in the Arabian Sea coastline of West Pakistan apparently took place sometime around the middle of the second millennium B C. This resulted in a disastrous increase in the already serious floods in the major river valleys with the subsequent rise of the underground water table, contributing to an increase in the soil salinity to the point where it was impossible to sustain the population of the vast urban settlements. The economy must have decayed rapidly; the Harappans were forced to migrate gradually to more fertile territory. There is now incontrovertible archaeological evidence that the major population shift was to the southeast into the area of the Kathiawar peninsula, north of Bombay. Here the Harappans mingled with other indigenous populations and gradually there was a complete absorption and transformation of the remnants of the formerly great Harappan culture into what we are coming to recognize as a distinctive chalcolithic culture of Central India. The former capitals of Mohenjo-daro and Harappa were virtually abandoned and became easy prey for bandits from the Baluchistan hills.

The enemy of the Harappans was Nature aided and abetted by the Harappans themselves, who accelerated the spoliation of the landscape through improper irrigation practices, and by denuding the watersheds through overgrazing and deforestation. They would have eventually put themselves out of business through such malpractices—just as the Sumerians did in southern Mesopotamia—but the process was speeded up by a sardonic twist of the earth's surface. Thus ended one of the three earliest civilizations of antiquity—Indra and the barbarian hordes are exonerated.

The End of the Ancient Cities of the Indus

ROBERT L. RAIKES

Among those who are interested in the prehistory of the Indian sub-continent there is probably no subject that has given rise to more speculation than that of how the Indus Civilization came into being, unless it be that of how and when it met its end. This article attempts a new look at the latter problem from outside the discipline of archeology.[1]

When work on this article was started in 1961, discussions with a number of archeologists interested in the whole problem of the Indus Civilization led the author to believe that his views were entirely original if not positively unorthodox. Recent access to several reports in the fields of geology, paleontology and geomorphology has revealed that this is not the case. In 1956 M.R. Sahni drew attention to the evidence for changes of level in the Indus Valley (Sahni 1956:102-107) and suggested that these may have been a contributory cause of the end of certain of the Indus Civilization cities. More detailed references will be made to his opinion later.

In a recent article in this journal (Raikes and Dyson 1961:270-278) some anticipatory doubt was expressed regarding the dating of both the Baluchistan peasant cultures and the Indus Civilization.

The Indus Civilization is generally referred to by archeologists as the Harappa Culture or civilization and its people as Harappans. These latter terms will be used throughout this article.

Brief Summary of Present Knowledge

The origins of the culture are not known, nor what became of its people after the destruction or abandonment of its cities. It is believed by many that it was imported from outside the Indus Valley, perhaps from outside the subcontinent, but there is insufficient evidence to say whether it started as a result of a mass immigration of new people or of ideas brought in by a few and imposed upon an autochthonous population. It is even possible that it developed as a local culture without significant outside influence.

It is deduced that the Harappa culture was already flourishing at the time of Sargon of Akkad (Wheeler 1953) from the discovery in Sumerian levels of that time of steatite seals showing evidence of Harappan contacts or influence and from the discovery at Moenjo Daro of Sumerian-type cylinder seals. The date of this contact would be about 2350 B.C. and it is generally considered that the Harappa culture started in about 2500 B.C. (Wheeler 1961).

It is relevant to note that E.J.H. Mackay quotes recent (1938b:7; 1948:147) discoveries by Dr. Frankfort at Tell Asmar in Iraq as indicating that the *upper* Harappan levels of Moenjo Daro were probably contemporaneous with the Dynasty of Akkad, i.e. 2500 B.C. The depth at Moenjo Daro down to which cylinder seals of Sumerian origin or influence were found seems to correspond with the phase known as Intermediate II (Piggott 1948:28).

There is a mass of physical remains of the Harappa culture in Pakistan (including Baluchistan) and in India, but despite this very little is known about it. The script carved on the many stone seals has not been deciphered; there is little evidence regarding the ethnology of the population; and the religion of the people can only be guessed at.

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We know that at one time or another the Harappa culture covered a vast area of the countries that are now Pakistan and India—an area bounded in the northeast by the Himalayas, in the southwest by the Arabian Sea, in the west by Baluchistan, extending in the east to as far as the Ganges-Jumna Doab, and in the southeast to Saurashtra in India. Most of the remains known at present are concentrated in the Indus Valley. While we know the approximate geographical extent of the culture, we are not necessarily entitled to assume that the whole of this area was occupied simultaneously. Indeed it is recognized that the southeastward extension into Saurashtra may have represented a later phase (Wheeler 1961:250) and other parts may have been occupied successively rather than simultaneously.

Excavations at Moenjo Daro, Harappa, Kot Diji and Chanhu Daro have revealed a very high level of material civilization, and the artifacts of all kinds—pottery, ornaments, bricks, weapons and implements of bronze and flint, seals, etc.—found in these excavations or on the surface at many other sites, show an extraordinary degree of uniformity. This uniformity extends both in time and space as evidenced by stratigraphy and geographical distribution. Taken in conjunction with the presently accepted estimates of the duration of the civilization, it suggests a high degree of authoritarianism, coupled with a degree of stagnation for which parallels would be hard to find (Piggott 1950:138-139).

The opinion has been expressed that the end of the civilization, or at least that of its principal cities, was due to invasion, perhaps by barbarians from the mountains of Baluchistan (Piggott 1950:214-242). At one time there was a general tendency to equate these invaders with the first raiding bands of Aryans whose exploits are sung in the Rigveda (Piggott 1950:244-248). There is still support for this view (Wheeler 1961:249). Recently some support has developed for the view that some time may have elapsed between the end of the Harappa culture and the coming of the Aryans. But whatever the cause, the date of the end of the Harappan occupation of the principal cities has been put at about 1500 B.C. (Wheeler 1953:89; 1961:243). Although identification of Moenjo Daro and Harappa (particularly the latter) with the fortified cities referred to in the Rigveda is no longer generally accepted, the effect of such earlier identification seems to have persisted and may have influenced more recent thinking in the direction of accepting a long duration for the cities.

Existing Opinion Open to Doubt

That Moenjo Daro and Harappa ceased to be occupied by Harappans is certain and that the end was comparatively sudden seems well established. The inference, however, drawn from a comparatively small number of skeletons of men who appear to have died violently, that the end of the civilization was due to invasion is surely too sweeping. The men whose skeletons have been found undoubtedly died violent deaths and their deaths occurred at about the end of the Harappan occupation, but it does not follow that they were killed by invaders who overthrew the whole civilization. The small number of skeletons would be equally explicable in terms of rioting or civil commotion of the kind that has characterized social revolution throughout history. There are no doubt other possible explanations such as minor fights between factions or a minor and localized raid by barbarians from the mountains or elsewhere. In fact, we do not know the cause of these deaths, whether they were connected with the end of the city, or, if they were so connected, whether they are direct evidence of the end itself or merely secondary symptoms of some other and quite different reason for its downfall. For some reason or reasons a civilization that had endured apparently for centuries disintegrated at Moenjo Daro. If it was overthrown by invaders, by civil dissension, by marauding attacks from the hills, it still remains to explain why it should have been unable to resist attack or to police itself.

Before considering possible reasons for the end of the Harappa culture or what may have contributed to it other than those already advanced by archeologists, it is worth considering more fully the stagnation referred to earlier. This stagnation, for which the case rests on an attested uniformity of material coupled with an unproved duration, is hard to believe. It is as if the material culture of France had remained unchanged in its minutiae from the time of Charlemagne to the French Revolution; or that of England from before the Norman Conquest until the Industrial Revolution. Its acceptance really depends on acceptance of the estimates of duration. If the evidence on which the latter have been based can be interpreted in

terms of a shorter duration, we would no longer have to believe simultaneously in a stagnation of ideas and in creative vigor: for creative vigor there must have been, to account for the founding of an enormous empire and its several reconstructions from nothing. The whole picture of Moenjo Daro would become more credible if it could be shown that occupation of the site was for a much shorter period.

That the possibility exists of a shorter occupation of Moenjo Daro by the Harappans is implicit in the fact already noted that Harappan-Sumerian contacts were assigned by Mackay to the upper and, therefore, later Harappan levels at Moenjo Daro (Mackay 1938b:7). With this evidence there seems to be a prima facie case on archeological grounds alone for a very much earlier final date of Harappan occupation at Moenjo Daro.

Whatever the length of occupation and whether there was stagnation or not, it remains to be explained, as noted earlier, why the city should have become fairly rapidly weakened to the point when it was unable to resist attack or to police itself. There are many possible reasons. There may conceivably have been a succession of years during which the annual Indus flood did not suffice for the food-growing needs of the people, but, the Indus flood being largely snow-fed from the Himalayas, this is unlikely; there may have been an epidemic, though in this case we would expect to find evidence of it in the form of hurried mass burials; there may have been social and moral degeneration, though if there were, the practically unchanging styles of building, art, utensils and the like existing until close to the end would indicate that it occurred with an extraordinary and unlikely rapidity which would itself require explaining; the "empire," like others since its time, may have become unwieldy and splintered; there may have been difficulty in obtaining the equivalents of modern strategic raw materials, particularly copper and tin.

None of these reasons has, as far as is known, the support of archeologists and none except the first two would be a sufficient explanation by itself; and there is no evidence for them. In regard to the others, one asks: Why did degeneration occur? Why would the unwieldiness of the empire of itself hasten the end? Why should raw materials have become difficult to obtain?

Before considering the answers to these questions and the explanation put forward for the sudden weakening of a powerful and well-organized civilization, let us reconsider the evidence for a long duration.

Review of Present Evidence for Dating of Harappan Culture

Archeological evidence in the form of accurate stratigraphy is lacking as regards the end of the culture. Nothing comparable with the Harappa-Sumeria link in about 2350 B.C. exists to date the more recent Harappa levels at Moenjo Daro (unless indeed that link relates, as Mackay has suggested, rather to the end of the Harappan occupation of Moenjo Daro than to the beginning).

The evidence, such as it is, is mainly indirect. It can be summed up briefly as follows: Excavation at Moenjo Daro showed as many as seven Harappan occupation levels, some of them separated by layers of flood silt. Consideration of the infrequency of catastrophic floods that could bury a whole city under as much as six feet of silt and over-liberal estimates of the time required to rebuild, when taken in conjunction with the original tendency to think of the end of the culture as coinciding with the arrival of the Aryans, has supported the belief in a long occupation of the site. It has been recognized from the outset that accumulation of silt was not confined to the four periods revealed by the only recorded stratigraphy (Piggott 1948:28) and marked on the published section as "flood silt." Sir John Marshall, who carried out the original excavations at Moenjo Daro, wrote: " the way in which the river has spread its alluvium over the whole expanse of plain is well demonstrated by the discovery of ancient brick remains nearly 30 feet below the present surface . . . " (Marshall 1931:7).

The basic assumption has been, in effect, that this silt was deposited by floods; that 30 feet of silt and seven rebuildings, which corresponded with seven inundations by floods, must have required the agency of very exceptional and, therefore, very infrequent floods; and that all this required a very long time.

The estimate of a long duration for the Harappa Culture as a whole has been lent support by the discovery of settlements of the culture in Saurashtra where, as Wheeler remarks (Wheeler 1961), the typical Harappan ware is found sometimes mixed with the

black-and-red ware typical of the latter period in Indian prehistory.

Critical Examination of the Present Evidence

Before examining the basic assumption just referred to, we should consider in passing the evidence from Saurashtra. The mixture of wares may be an indication of the coexistence of the final phase of the Harappa Culture and of the initial phase of the black-and-red ware, but it is not absolutely conclusive, nor, for that matter, is the date of the initial phase of the black-and-red ware. The existence of a sterile layer in a mound or buried settlement separating two distinct cultures is positive evidence of an interval of time between the end of one and the beginning of the other. No accident of nature could cause the existence of a sterile layer separating two distinct cultures that were, in fact, coexistent. An apparent mixture of cultures, however, can be explained in several ways and need not be positive evidence that the cultures coexisted. For instance, the use by a succeeding culture of collapsed mud-brick material mixed with potsherds of an earlier culture may, when the buildings erected by the latter collapse in their turn, result in an apparent mixture of the two types of pottery etc., even though there may have been some centuries during which the site was abandoned.

Returning to the basic assumption referred to above, it is necessary first to define the terms used. A *flood* in this context is taken to mean abnormal river flow at such a rate of discharge that the capacity of the river bed is exceeded so that excess flow has to be accommodated by the flood plain. *Silt* is solid matter carried in suspension by the river and comprises particles of clay, true silt, and even fine sand when the velocity of flow is high enough. Generally speaking, the excess flow would, except locally, move very slowly in the general direction of flow of the river. Only in the original channel would consistently high velocities be experienced. Over the flood plain the velocity would be low and would depend on the gradient, the depth, and the roughness. Of these factors the depth would be most subject to variation. In a river such as the Indus, variations in gradient and roughness would be marginal. Even at a depth of only one foot of flood plain flow, and even at the remarkably flat gradient of the Indus Valley, there would be an appreciable velocity, though it might be low enough to ensure that some of the suspensed silt would be dropped.

It is just conceivable that a combination of high discharge, exceptionally long duration, and little variation of the high discharge rate might deposit silt uniformly and to a depth even as great as two or three feet, but it is unlikely. For such an exceptional concatenation of conditions to occur seven times is even more unlikely.

Obviously, a long investigation into the prehistoric silting regime would be an unrewarding occupation; it would have to depend on so many assumptions, all incapable of verification, that the results of the investigation could hardly fail to be misleading. Fortunately we do not need to go to so much trouble in order to cast doubt on whether floods, in the sense defined earlier, caused the inundations of Moenjo Daro.

These inundations have caused a general rise in the level of the Indus flood plain that has buried the earlier Harappan levels under at least 30 feet of silt. The lowest level explored so far does not necessarily go back to the earliest occupations of Moenjo Daro, but it does not make much difference to the present argument whether this level is dated to 2500 B.C. or somewhat later. The topmost Harappan occupation level coincides approximately with the flood plain level so far as the "lower and outlying parts of the city" (Marshall 1931:1) are concerned; this is of course the most recent Harappan level, that which corresponds most closely with the end of the culture. Taking the archeologists' estimate of approximately 1000 years duration, a very strange fact emerges: during 1000 years of occupation of the city, 30 feet of silt were deposited; during the subsequent nearly 3500 years, no further silt has been deposited. It is even possible that there has been some lowering, by erosion, of the flood plain during this second much longer period. Clearly there is an anomaly here that cannot be simply explained in terms of abnormal floods, however irregular their occurrence.

For flooding to continue to be tenable as a cause of the inundations of Moenjo Daro, it would be necessary to suppose that the flood regime of the Indus underwent an enormous and extremely sudden change at the end of the Harappan occupation of the site. The flood regime of the Indus, being dependent largely on snowfall and snowmelt in the Himalayas, is for that very reason independent of local climatic variations in the plains; for the snowmelt pattern of the Himalayas to change, it would be necessary to postulate a major and sudden climatic change which could hardly have

been confined merely to the Indian sub-continent and of which there is no conclusive evidence from other parts of the world. In any case there is no reason to suppose that a sudden change in the flow regime did occur, for, despite man's control of the Indus by barrages and in other ways in recent years, the river is still every so often liable to large floods. It is relevant to note that these floods, in spite of much evidence of greatly increased soil erosion in the headwaters (and therefore of a probably higher silt load), do not cause sudden significant changes in the flood plain level.

One of the premises, on which has been based the estimate of a long occupation of Moenjo Daro, may therefore be entirely false, and it follows that the estimate itself may be wrong. The city may have been occupied for 1000 years; it may, on the other hand, have been occupied for as little as a century or two. In the latter case, the question of stagnation would not arise.

Need for Reconsideration of Dating

Unless, therefore, convincing archeological evidence can be adduced in favor of a very long occupation, the question of Harappan dating will require careful reconsideration. Since much of the very tentative dating of southern Baluchistan cultures is dependent on contacts with the Harappa culture, this will also require re-examination.

It is suggested that there does not exist an imperative need to account for continuous occupation of the areas of Pakistan and India in which the Harappa culture flourished. While there is evidence that Harappa immediately succeeded the Amri and Kotdiji cultures at those sites, there is as yet no conclusive evidence that the Jhukar culture immediately succeeded Harappa either at the type-site or at Chanhu Daro (Piggott 1950:222); and, while there may be some evidence at Lothal and Rojdi (in the Saurashtrian area previously referred to) that the Harappa culture was immediately succeeded by the producers of black-and-red ware, at Rangpur in the same district the evidence is of a break between the cultures (Wheeler 1961:250).

New Evidence for the End of the Harappan Culture

As has already been noted earlier (Sahni 1956:102-107), the possibility of changes of level has been put forward as a contributory cause of the end of certain cities of the Harappa culture. Sahni, in the publication referred to, drew attention to the occurrence in 1819 near Hyderabad, Sind, of a sudden local uplift of part of the flood plain of the Indus causing flooding of about 2,000 square miles which endured for about two years (Fig. 40.1). This uplift of a strip of land about 50 miles long, on an average about 15 miles wide, to an average height of some 15 feet above the flood plain, occurred apparently as a result of an earthquake (or perhaps one should say its appearance was the cause of the earthquake). Such an occurrence near Moenjo Daro could very easily have accounted for the still-water flood conditions necessary to explain the great depth of silt.

Sahni refers to evidence in the Indus delta of both uplift and settlement and affirms his belief that major tectonic movements may have partially accounted for the end of some of the Harappan cities. He prefers, however, to ascribe the flooding of Moenjo Daro to the bursting of temporary natural dams in the Indus headwaters, even though such flooding would probably not have accounted for the silt deposits at Moenjo Daro without local uplift to cause ponding; and if there were uplift it would not need exceptional river flow to cause ponding. Collateral evidence comes from Baluchistan and is both geological and archeological.

During the winter of 1960/61 an expedition from the University of Pennsylvania discovered a site of the Harappa culture at Sotka Koh near Pasni on the Mekran coast (Dales 1962b:86). Almost simultaneously, and quite independently, the author discovered another site of the Harappa culture at Bala Kot near Sonmiani about 50 miles from Karachi on the way to Bela. One other coastal site, that known as Suktagen Dor (Stein 1931:60), had already been known as a site of the Harappa culture. It is situated at the extreme southwest corner of Baluchistan practically at the Iranian frontier, and has always been something of a mystery because of its great distance from most of the other known sites of the culture. These three sites, all of which are now at some distance from the sea both vertically and horizontally, suggested the idea of a chain of coastal sites westward from Karachi where, in the hinterland a few miles to the north of the city, another site, said to have Harappan affinities, exists at Mango Pir.

Study of the available records shows that the southern part of the Indus Valley flood plain, roughly that part from Amri and Chanhu Daro to the mouth,

contains no identified Harappan sites despite very extensive search (Majumdar 1934). The same absence of recorded Harappan sites marks the whole of the Las Bela plain from near the little town of Bela to the sea. The known sites of the Harappa culture from Amri to Karachi follow the extreme edge of the foothills of the Kirthar range, comprising such sites as Tharro hill, or are situated at short distances from the plain along one or the other of the small rivers draining that range.

Examination of the aerial photographs of the Mekran coast shows raised prehistoric beaches at various points running approximately parallel with the present beach line. These beaches are particularly visible in the neighborhood of Gwadar and Ormara, and it is possible to distinguish about ten of them. These prehistoric beach lines are evidence that at some period in the past the level of the sea was higher in relation to the land, or that since that time the level of the land has risen relative to that of the sea. This upward movement of the coast relative to the sea may have occurred hundreds of thousands of years ago or merely a few thousand years, and it must be admitted that some of the geologists consulted on the subject tend to think in terms of hundreds of thousands of years. This may be due to what appears to be a certain reluctance on the part of some geologists to consider anything more recent than about 20,000 years old as geology. Evidence will be quoted later in support of recent uplift.

The map (Fig. 38.1) shows the distribution of known Harappa sites. The area shaded with diagonal lines on the map represents those parts of the coastal strip and of the Indus and Bela plains that could have been under the sea at the time of the Harappa culture, if the changes in coastal level took place after that time.

There is nothing particularly original about the idea that the Harappa culture made use of waterways such as the Indus as main highways for commerce, and the possibility of coastwise trade has long been recognized. The map, however, gives it a new emphasis because, assuming a coastline approximating that shown, practically all of the sites of the Harappa culture would have been on a sea or river. An exception might have been Judeir jo Daro, north of the modern Jacobabad, but there is at least a possibility that changes of ground level and a higher water table might have caused the reappearance of the combined perennial flows of the many small rivers draining through the Kacchi plain at the south end of that plain. Today the waters of the Nari, Bolan, Mula etc. disappear underground soon after reaching the plain, but they contribute to a water table which is even today only 40 feet or so below the surface at Judeir jo Daro. The coastal sites in Baluchistan are all situated on what are still important caravan routes into the interior.

The foregoing may be considered as constituting an archeological case for changes in the coastline due to changes of level (in this case uplift) along the coast and in the lower Indus Valley. The evidence may not be entirely convincing, but an expanse of sea where there is now dry land, in the areas shown in Fig. 38.1, would immediately explain the otherwise rather strange distribution of Harappa sites.

As regards geological evidence, it has been suggested that some at least of the prehistoric beaches are the result of recent coastal uplift (Asrar Ullah 1953:4-5; Moh'd Ismail Siddiqi 1953:2, 3, 5; 1956:5, 31, 32; Sahni 1956:101-107; Harrison 1941:12; Pendse 1946:144; Snead 1963[2]). Asrar Ullah in particular equates some of the raised beaches with gravel terraces found in the inland valleys. He refers to five terraces in one particular valley which he interprets as marking intervals of rest between periods of movement affecting land and sea levels, the whole process of movement having occupied a comparatively short time. He bases his opinion of a short duration for this period of movement on the fact that none of the terraces shows any signs of having completed an erosion cycle.

In the intermontane valley of Ornach, there is possibly corroborative evidence of a recent date for the movements that resulted in the formation of these terraces. The site of Nindowari, having Harappan affinities (de Cardi 1959:24), is now situated, together with what appear to be ancient terraced fields that may have been associated with it, some 25 to 30 feet above the level of the perennial Kud river. Similar conditions apply to at least two other sites of the chalcolithic period or earlier. The site, however, of the Londo culture (de Cardi 1951) at Shori Damb is situated only just above the bed level of the Turkbar Nala and the same applies to at least one other site of the Londo culture in the Ornach valley. It appears possible that the site at Nindowari was isolated from its former water supply by down-cutting whereas some sites of the Londo culture do not appear to have suffered in the same way. The little mound of Kulli (Londo culture), a mile or so upstream of Nindowari, appears at first sight to contradict this statement, as it is situated on the gravel terrace at much the same height above the river as in the case of Nindowari. However, there are

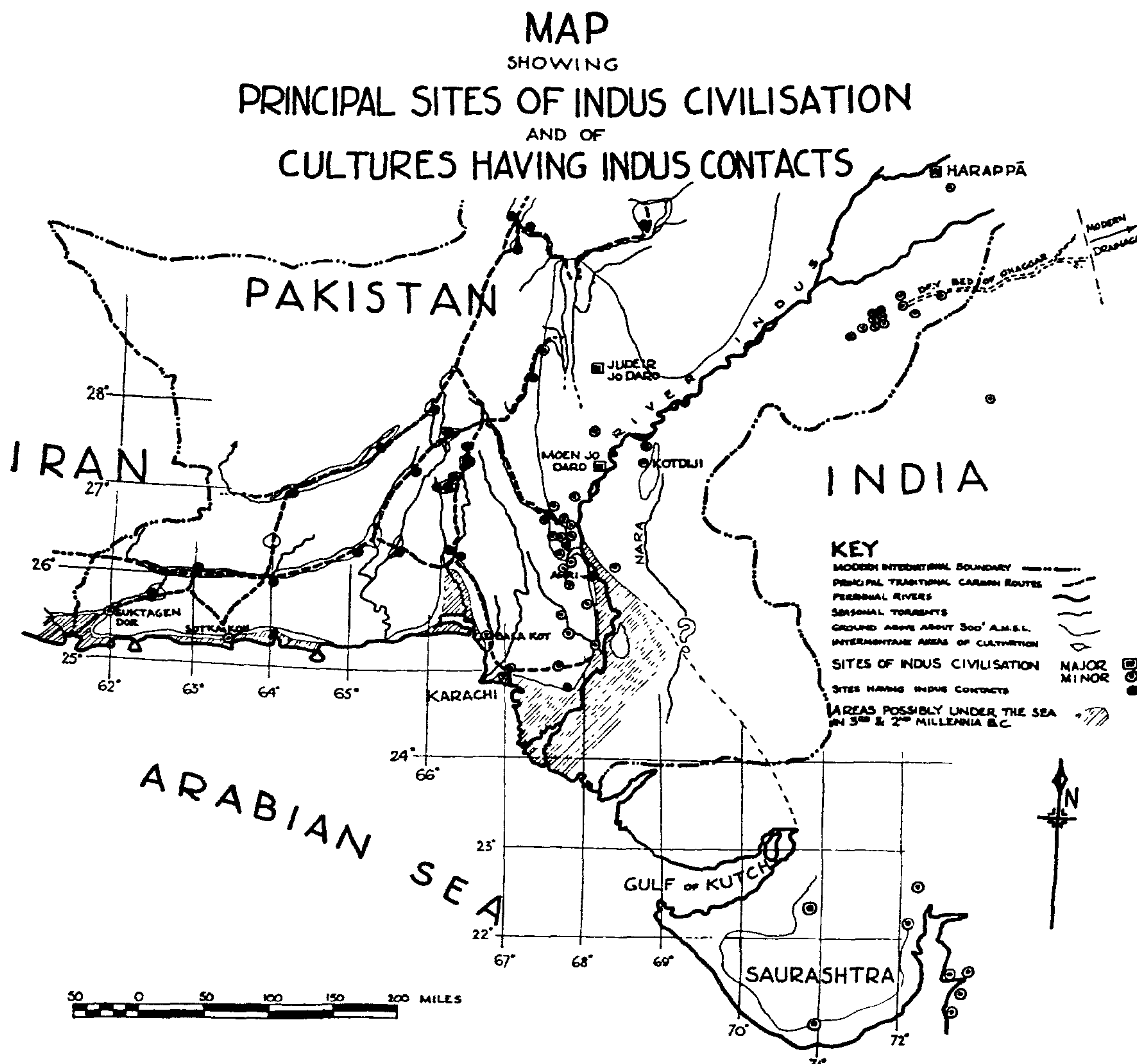


FIGURE 38.1. *Map of the Indus Valley showing sites and flooded areas.*

no ancient terraced fields around Kulli, and it is probable that its inhabitants cultivated the existing alluvial flat, below the gravel terrace, which is at present river level. Down-cutting is a phenomenon for which several explanations can be offered, but in view of Asrar Ullah's opinion, it seems most probable that at Ornach it was due to local uplift near the outlet of the valley. The Londo culture has been tentatively dated to 1200 B.C. (de Cardi 1951) while Nindowari must have been occupied during at least some part of the Harappan occupation in the plains and probably during an early phase (de Cardi 1959:24). It seems reasonable, therefore, to infer that uplift occurred sometime during the occupation of Nindowari by Harappans or by people who were in contact with Harappa and before the arrival of the people responsible for the Londo culture; it is tempting to infer that the uplift and consequent isolation of Nindowari from its source of water was the principal cause for the abandonment of that site. It is also probable that uplift of this magnitude would have been accompanied by earthquake shocks that would in any case have destroyed Nindowari.

Apart from a few Carbon-14 datings of material from sites near Quetta further to the North (to the early part of the third millennium B.C.) the dating of the peasant chalcolithic cultures of Baluchistan depends partly on Harappan contacts and partly on

typological similarities with peasant cultures in Iran. The question of relative dating of such Baluchistan cultures as Nal, Kulli, Togau, etc. is still an open one, but, for Baluchistan in general, there has been a tendency (Piggott 1950) to consider that the end of these cultures, or, at any rate, of the later ones, coincided with a "time of troubles" that occurred at about the time of the end of the Harappa civilization. They are, therefore, believed to have survived until perhaps as late as 1500 B.C. As their affinities with Iran in some cases go back well into the third millennium B.C. or even earlier, their ending in about 1500 B.C. argues a very long occupation or sequence of occupations in the mountains. This is difficult to accept (for the same reasons as stated before for Moenjo Daro) when one considers the excellence of the fabric and decoration of, for instance, Nal pottery. It is almost impossible to believe that this very beautiful pottery was produced with little significant change for generation after generation, in virtual isolation in the mountains.

On the whole it seems to fit in much better with archeological evidence and with that of geology to hypothesize that these peasant cultures in the hills came to a fairly sudden end due to the same causes that destroyed Moenjo Daro, and that a fairly early date, perhaps even as early as 2300 B.C., should be considered for this period of tectonic activity. The evidence cited (Piggott 1950) of layers of burning at many chalcolithic sites in Baluchistan and tentatively explained as being due to the arrival of invaders possibly from the West is equally explicable in terms of earthquake damage. Much of the loss of life in the earthquake that devastated Quetta and some of its neighboring villages in 1935 was due to fire. Inflammable roofing material fell onto the fires that, in winter, are kept burning, all the time and, at other seasons, most of the time. There does not seem to be any convincing reason why between say 2300 B.C. and 1100 B.C. the whole of Baluchistan should not have been virtually abandoned, the peasant populations having reverted to nomadic life as a result of natural disasters. In support of this could be cited the present semi-nomadic population of the area who use little in the way of materials that will survive to interest the archeologist of the future.

CONCLUSIONS

It would be impertinent for a layman to assert that the archeologists who have studied this problem are mistaken in their deductions. The substitution of one explanation for the inundation of Moenjo Daro by another does not of itself invalidate presently accepted estimates of the duration of the Harappa culture. The uplift and associated earth movements to which the destruction of the culture, and of the Baluchistan peasant cultures, may be attributable may as well have occurred in about 1500 B.C. as at a much earlier date.

It is, however, suggested that the archeologists *may* be wrong and that the whole subject merits fresh study and a new approach. Whatever may be the verdict on purely archeological grounds, it must be accepted that the hitherto accepted interpretation of the geomorphological evidence is open, at the very least, to grave doubts.

Any new approach, therefore, should not be hampered by the present belief that geomorphological evidence requires a long duration of the culture.

Work currently being carried out in Baluchistan and recently completed in Sind[3] is expected to throw light on the beginnings of the Harappa culture, its relationship to the Baluchistan Peasant Cultures, and on the sequence of the latter. A fairly precise relative chronology, therefore, for Harappa beginnings may soon be available. The question of how and when it ended is still unresolved.

If the views put forward in this paper are accepted, we have immediately a logical and easily understood explanation of how at least part of the Harappa culture met its end: the part represented by the sites of the culture in Sind and Baluchistan. Uplift, almost certainly accompanied by more or less violent earthquake shocks, would not only have caused destruction of cities and settlements but would have disrupted the system of river and coastwise communications on which the commercial life of the culture must have largely depended. Damage to the cities and settlements in the Indus Valley would have been due rather to flooding than to the shocks.

Instead of the apparent stagnation of the culture, we would have to accept rather a dogged determination and a creative energy sufficient to account for seven rebuildings of Moenjo Daro. A stage would have been reached when the people, deprived of their sea communications with the outside world and with Baluchistan, would have had to resort to the difficult caravan routes through the hills of Baluchistan. Finally, a point would have been reached when it would no longer have been considered worth the trouble and expense of rebuilding in Sind. The possibility is at least worth

consideration that, towards the end of the occupation of Moenjo Daro, it was decided to move the capital to Harappa. For the idea of successive capitals is surely more acceptable than that of twin capitals. At the same time a part of the culture may well have migrated to Saurashtra.

As the successive destructions of Moenjo Daro may have all occurred within a short span of years, as the evidence of Asrar Ullah seems to suggest, the difference in time between the occupation of Moenjo Daro and Harappa may well be too short to be detectable by Carbon 14 tests: and the same may apply in the case of Saurashtra settlements. One must also bear in mind the possibility that Harappa was occupied before Moenjo Daro.

At a later stage (and no one, on the basis of present evidence, could guess how much later) the group of settlements centered on Harappa itself, most of which are in the flood plain of the now dry Ghaggar River, may have been evacuated in favor of new settlements such as Rupar towards the Ganges-Jumna Doab, due, perhaps, to the drying up of the Ghaggar.

In the mountains of Baluchistan, much the same sequence of events may have occurred except that in the rigorous living conditions there, with the much greater difficulty of mass migration through waterless, inhospitable, mountain desert, the disasters may have led to the extinction of the cultures. This could have been due in part to exhaustion of their own resources and in part to the sudden removal of the "colonial" cultural influence of the Harappans. In isolation, and weakened by natural disasters, the population may have decreased fairly rapidly, leaving Baluchistan peopled, if at all, by semi-nomads eking out a precarious existence around the ruins of their former civilization.

All such ideas must remain, for the present, pure speculation; they represent, in any case, only some of many possible explanations.

It may be considered an impertinence for someone who is not an archeologist to suggest specific points towards which research might be directed, and such suggestions are put forward, therefore, with diffidence. For what they are worth they are: precise dating of the earliest black-and-red ware levels in Saurashtra sites; the enormous time gap at present accepted between Iranian chalcolithic prototypes and their typological descendents in Baluchistan; the possibility of successive occupation of the various groups of Harappan sites (Sind and Baluchistan, Punjab, Ganges-Jumna Doab, Saurashtra); and new excavations at Moenjo Daro and one of the Mekran coastal sites directed at finding dating evidence for the later levels.

At the same time, study of certain hitherto neglected sites in Baluchistan might be profitable. In the tracts of Wadh and Ornach there are at least three sites,[4] characterised by a prolific microlithic flint industry; a stone hut was found at one and (on the surface at least) a very few potsherds of an indeterminate nature. It is just possible that these may represent settlements of the degenerate chalcolithic peasant cultures during the "dark ages" that may have preceded the arrival of the Londo culture. It is perhaps more likely that these settlements antedated the chalcolithic settlements;[5] either way they are of interest.

An attempt has been made to show that the evidence of geomorphology and hydrological engineering supports the claim of a short duration of the Harappa culture whose end was encompassed in a way different from that hitherto accepted. So far as is known, the purely archeological evidence is not inconsistent with this view. It is hoped that the possibility of a short duration will be treated with at least as much respect as has been accorded so far to the evidence of a long duration, bearing in mind that the non-archeological evidence for the latter must now be treated as, at best, suspect.

NOTES

[1]This article owes its existence to the encouragement given to the author by Professor Dales and its present form to the generous and friendly advice of the editorial staff. Editor's note: Mr. Raikes chose to adopt the spelling of Mohenjo daro which is current in Pakistan. That is "Moenjo daro" for "Mohenjo daro " In fact his is the more accurate choice. See Sorley (1959:111) for a discussion of the derivation of the name. However, for the sake of tradition and continuity the original spelling "Mohenjo daro" has been retained in a majority of the articles in this book.

[2]For much extremely valuable information in these fields the author is indebted to Mr. Rodman B. Snead, now Assistant Professor of Geography at Clark University, Worcester, Mass. who carried out in 1960-1961 a study of the Physical Geography of the Las Bela coastal plain for the Institute of Coastal Studies of the

University of Louisiana. He has very kindly made available for study the relevant parts of his forthcoming publication on the subject.

[3]Excavations were carried out from 1959-1963 at Amri in Sind under Prof. J.-M. Casal by "La Mission Archéologique Française de l'Indus" with the object of establishing, among other things, relationship of the Amri Culture to Harappa. During the past season (1962-1963) Prof. Casal has been excavating at Nindowari in the Ornach Valley with the object of establishing the relationship between Baluchistan Chalcolithic Cultures and Harappa.

[4]Karez damb first reported by the author in 1956 and Hurro damb found by him in 1961 are both in Ornach Valley. Bandu damb in the Wadh tract is the site referred to by Sir Aurel Stein as Abdul—But (Stein 1931:174).

[5]Miss de Cardi, writing of Nindowari and Kinneru Mounds in Ornach, mentioned the lack of the usual Harappa shell or clay bangles. The author, in 1961, picked up on Karez damb near Kinneru part of a shell bangle. If, as is possible, Karez damb was already an antiquity when Kinneru was occupied by Harappans or by people with Harappan affinities, it is tempting to surmise that perhaps Miss de Cardi was not the first lady archeologist to be interested in Karez damb.

The Decline of the Harappans

GEORGE F. DALES

Four thousand years ago the world's first three civilizations were flourishing. The Sumerians of Mesopotamia and the Egyptians of the Nile valley are reasonably well known to us. The third civilization embraced an area more extensive than either Egypt or Mesopotamia, yet it is far less known. Its most impressive remains—the dead cities of Mohenjo-daro and Harappa in the Indus valley of what is now West Pakistan—were first excavated four decades ago (see Piggott 1953). Our knowledge of this remarkable culture of South Asia, which is called the Harappan civilization, has been limited until recently to what could be gleaned from archaeological findings at these two sites, mainly because the written records of the culture are scanty and not yet deciphered. New discoveries in both Pakistan and India, however, are now adding much to our understanding of certain events in Harappan times.

In the past few years many Harappan towns and villages have been discovered well outside the civilization's nucleus in the Indus basin, indicating that the Harappan state extended much farther than earlier investigators had realized. It is now known that Harappan authority reached westward at least to the modern border between Iran and Pakistan, that it touched the foothills of the Himalayas to the north, even extending to the headwaters of the Ganges, and that it stretched southward along the west coast of India as far as the Gulf of Cambay to the north of modern Bombay. The Harappan civilization thus controlled or dominated a triangle roughly 1,000 miles on a side (Fig. A). A series of carbon-14 dates from Harappan sites along the coast of India also shows that many of these southerly towns and trading posts had continued to be occupied much later than the sites in the Indus valley. This and other bits of unexplained evidence have raised doubts concerning a fundamental hypothesis about the Harappan civilization: that Harappa and Mohenjo-daro had been sacked, and the Harappan civilization liquidated or absorbed, by the Aryan invaders who presumably brought the Indo-European language and culture to prehistoric India sometime during the second millennium B.C.

With the intention of learning more about the life and death of Harappan civilization the University of Pennsylvania and the Pakistan Government Department of Archaeology agreed on a joint reopening of the Mohenjo-daro site during the winter of 1964-1965. The expedition undertook, as part of a three-year program, to determine the total depth of the site's deposits of human occupation, something earlier workers had been unable to establish because groundwater lies only 15 feet below the surface of the plain at Mohenjo-daro. Efforts were made to devise some means of excavating these flooded occupation levels and to analyze evidence at the site, in the form of abundant accumulations of water-deposited silt, that the city had more than once been exposed to major floods.

During its mature period, from a few centuries before to a few centuries after 2000 B.C., the city of Mohenjo-daro housed an estimated 40,000 inhabitants in an area about a mile square. Today its ruins consist of two parts; a western mound that contains the so-called citadel is separated by a broad gully from a much larger eastern mound that contains the lower town. At the southwest corner of the lower-town mound an undisturbed area rises some 35 feet above

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the surrounding plain. In 1964 this area was selected for excavation in the hope of obtaining a sequence of stratified materials that could be correlated with the artifacts unearthed by earlier expeditions.

As a start it was decided to sink drill holes straddling the selected area to discover the depth of the earliest occupation levels. Core samples were collected at two-foot intervals; before the drills struck sterile soil 39 feet of core containing evidence of occupation had been raised to the surface. Thus the total depth of occupation in this part of Mohenjo-daro is 74 feet, about the equivalent of a seven-story building. Of the entire deposit, the deepest and therefore the earliest 24 feet (or almost a third) is still not available for study because of groundwater.

In the 1920s and 1930s, when the first digging was done at Mohenjo-daro, carbon-14 dating techniques were unknown and the only firm evidence of the city's antiquity came from the discovery of a few Harappan artifacts, principally stone seals, in ancient Mesopotamian sites. The Harappan seals found in reliably dated Mesopotamian strata belonged to a period extending from about 2350 B.C. to about 1800 B.C. The early investigators concluded that Mohenjo-daro and Harappan civilization in general had flourished during this same period. The carbon-14 dates now available for a number of Harappan and pre-Harappan sites tend to confirm this dating but also suggest that from 50 to 100 years might be added at each end of the period. When several new carbon samples collected from late levels at Mohenjo-daro in 1964-1965 are analyzed, they should establish the date when that city—and civilization in the southern Indus valley—met its end.

Before discussing why and how Harappan civilization declined, something should be said about its origins. No formative phase, or early stage, of Harappan culture has yet been positively identified in the archaeological record of South Asia, although current excavations in Pakistan and India are beginning to yield some clues. Numerous pre-Harappan cultures have been found in the hills and valleys of Baluchistan, to the west of the Indus valley. Pre-Harappan groups also lived in the Indus valley itself just before Harappan culture appeared there in mature form. The Baluchistan sites have strong ties with Afghanistan and the Near East, but their relation to the origin and development of Harappan civilization is little understood. The same is true of the civilization's precursors in the Indus basin. At sites such as Amri, Kot-diji and Kalibangan materials belonging to the mature Harappan phase are found mixed with materials from late phases of the indigenous cultures. Such findings suggest that Harappan civilization arrived full-blown from some other area. At the same time earlier levels of these and other pre-Harappan Indus sites contain objects that are characteristic of Harappan civilization in its maturity. These findings argue either for an on-the-spot evolution from one culture to the other or, at the very least, for heavy borrowing from the local inhabitants by the Harappans when they first settled in the Indus valley. Perhaps the 24 feet of waterlogged occupation layers at Mohenjo-daro, containing as they should a record of the city's earliest development, will help to illuminate this question of Harappan origins.

The problem of the decline and disappearance of Harappan civilization has been a matter of primary concern during the expedition's first two years of work. One of the discoveries made at Mohenjo-daro in the 1920s that is cited in support of the hypothesis that Aryan invaders destroyed the Harappan civilization was the presence of some 30 human skeletons in what appeared to be the upper levels of the site. The bodies had evidently been left where they fell rather than receiving burial; this seemed a dramatic archaeological confirmation of the postulated invasion and massacre. In 1964 the poorly preserved remains of five more bodies were unearthed in the upper levels of the excavation, only two to three feet below the surface of the mound. They lay in a narrow alley amid an accumulation of collapsed brick, broken pottery and ash. The period to which these dead belong is evidently late, but it has not yet been precisely determined. The fact that they had not had any normal kind of burial suggests that all five were the victims of some common disaster. Nonetheless, we are reluctant to believe that either the earlier discoveries or our own support the hypothesis of an Aryan invasion. For one thing, no one has any exact knowledge of the date when the Aryans first entered the Indus valley area; they have not yet been identified archaeologically. For another, the sole purpose served by the invasion hypothesis is to explain the demise of Harappan civilization. If evidence can be found that Mohenjo-daro declined for other reasons, the invasion hypothesis goes by the board. Such evidence, in the form of traces of catastrophic floods, is now being subjected to close scrutiny.

The presence of water-deposited silts at Mohenjo-daro had been recognized by early workers at the site as an indication that floods had played a role in the city's history, but no one suggested that the silts represented anything more significant than periodic brief overflows of the Indus River. Excavators who later worked at such sites to the south of Mohenjo-daro as Amri and Chanhu-daro found abundant evidence of flooding in these areas also. It was not until 1940, however, that anyone suggested a relation between the archaeological evidence of ancient floods and a number of topographic and geological anomalies of the Indus valley. In that year the Indian paleontologist M.R. Sahni noticed silt deposits perched many feet above the level of the Indus plain near the city of Hyderabad in what is now West Pakistan. This and other evidence suggested to him that the area's ancient floods had not been mere river overflows but events on a far larger scale. Major tectonic upheavals, Sahni proposed, might have blocked the Indus River from time to time; each such stoppage would have caused the gradual formation of a huge upstream lake that might then have persisted for decades.

Sahni's suggestion went virtually unnoticed until 1960. By that time two totally independent lines of research had led to the identical conclusion: natural disasters must have played a major role in the decline of Harappan civilization. In that year the University Museum undertook an archaeological survey of the Arabian Sea coast of West Pakistan. The field party discovered settlements of the Harappan era that had clearly been seaports but were now located as far as 30 miles inland. These displaced ports made it evident that the coastline in this part of Pakistan had risen considerably during the past 4,000 years, with the initial rise apparently having occurred during the Harappan period. In the same year Robert L. Raikes, a hydrologist, was conducting extensive surveys in southern Baluchistan and the lower Indus valley. Raikes's keen antiquarian interests led him to investigate the possibility that ancient topographic changes in the area might well be related to the decline of Harappan civilization. The mutual desire to combine archaeological evidence with the findings of another discipline led to a joining of forces. Raikes is now working with the Mohenjo-daro expedition as engineering consultant and is in charge of geological and hydrological investigations.

Just as Sahni had been puzzled by the silts near Hyderabad, so Raikes in 1964 sought an explanation for the thick silt deposits that are preserved in the ruins of Mohenjo-daro at points as high as 30 feet above ground level. The 1964 test borings that revealed the thickness of the underlying archaeological strata at the site showed that silt deposits also existed below the surface of the plain. When the layers above ground are added to those below, the silts sandwiched between the city's successive occupation levels span a vertical distance of 70 feet. A better explanation than occasional floods is obviously needed to account for such a multilevel accumulation

Raikes's preliminary research not only suggests that the dam-and-lake hypothesis proposed 25 years ago by Sahni is tenable but also singles out an area near Sehwan, some 90 miles downstream from Mohenjo-daro, as the most probable area of tectonic disturbance affecting the city. Both at Mohenjo-daro and at smaller sites between the city and Sehwan the silt deposits are of the kind characteristic of still-water conditions in a lake rather than the kind deposited by the fast-moving waters of a flooded river. Moreover, there is abundant geological evidence of rock faulting on a large scale near Sehwan. The faulting by itself could have raised a natural dam and turned the upstream portion of the Indus into a slow-filling lake. More probably, however, the same disturbances that caused the faulting were accompanied by massive extrusions of mud, aided by the pressure of accumulated underground gases. Such mud extrusions are not uncommon in Pakistan even today; for example, a number of mud islands abruptly appeared off the Arabian Sea coast in 1945.

Let us assume that some such barrier was thrown up near Sehwan. Thereafter the normal discharge of the Indus would not have reached the sea but instead would have accumulated in a steadily growing reservoir (Fig. 39.1). As the rising waters encroached on the valley's villages and towns, many small settlements undoubtedly disappeared below the surface and were completely obliterated by silt. When the waters approached such a major population center as Mohenjo-daro, however, it seems logical to suppose that efforts were made to protect the city. The archaeological evidence strongly suggests that large-scale community projects were indeed undertaken at Mohenjo-daro for this purpose. As an example, massive mud-brick platforms were erected and faced with fired brick, apparently with the objective of raising the level of the city safely above the lake waters. One such embankment, partially excavated by the expedition in

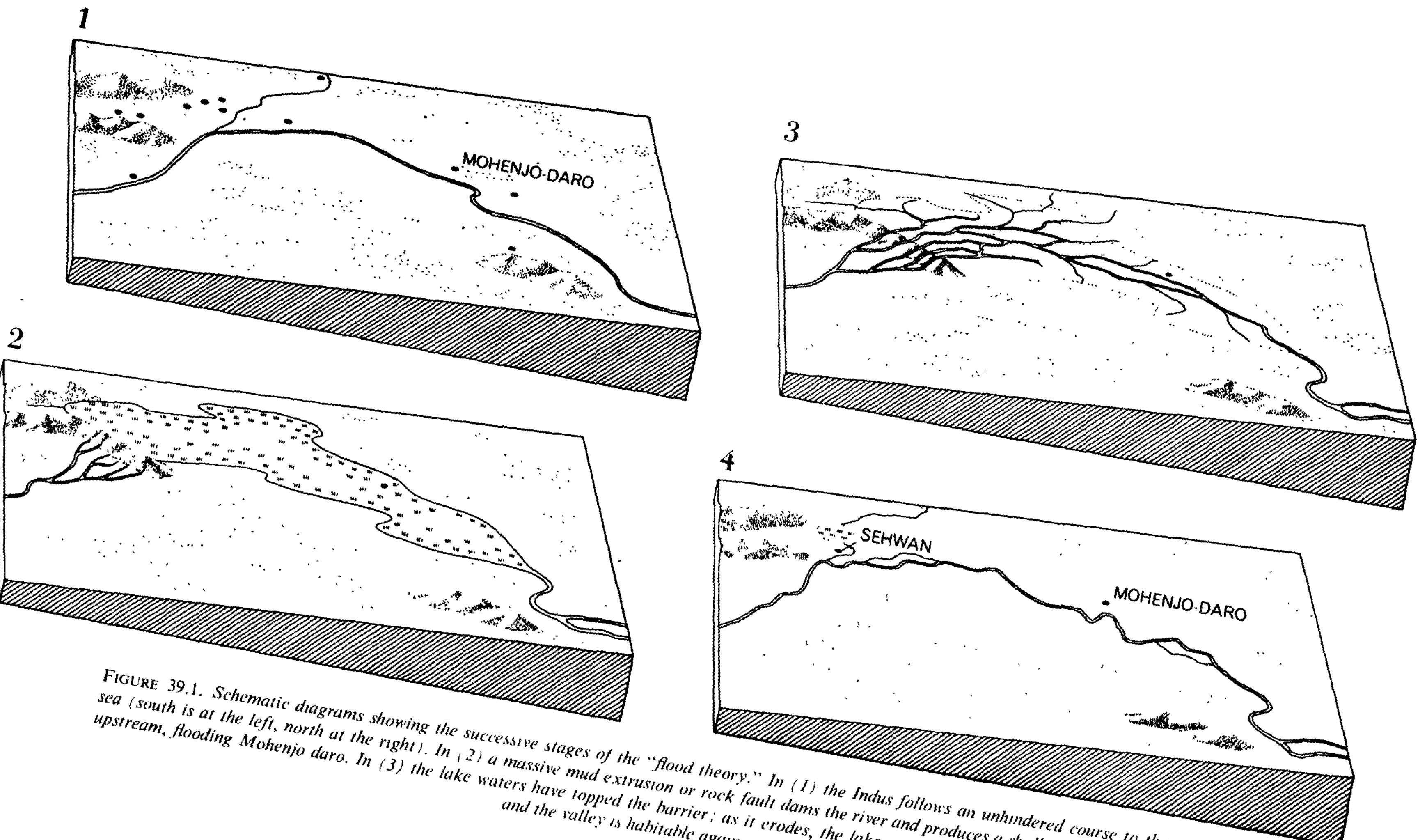


FIGURE 39.1. *Schematic diagrams showing the successive stages of the "flood theory." In (1) the Indus follows an unhindered course to the sea (south is at the left, north at the right). In (2) a massive mud extrusion or rock fault dams the river and produces a shallow, marshy lake upstream, flooding Mohenjo daro. In (3) the lake waters have topped the barrier; as it erodes, the lake empties. In (4) the cycle is complete and the valley is habitable again.*

1964, is some 70 feet wide and well over 25 feet high.

Eventually the waters accumulating behind the natural dam would have risen until they had spilled over it and begun to cut it away. Thereafter the Indus would have resumed its normal flow to the sea and re-erosion of the silt-covered floodplain would have begun. After each immersion the inhabitants of Mohenjo-daro found it necessary to rebuild or reinforce most of the city's buildings. Although they usually rebuilt directly on top of the older foundations and walls, they eventually encountered serious problems of decay and sinking. The ruins today dramatically illustrate the problems they faced.

Both the multiple layers of silt at Mohenjo-daro and the evidence of multilevel reconstruction suggest that the city was flooded in this prolonged and damaging fashion no less than five times and perhaps more. At present it is impossible to estimate just how long each cycle of lake intrusion and withdrawal may have lasted, but it seems doubtful that the duration of any one cycle would have exceeded 100 years.

Could such a series of natural catastrophes, rather than the Aryan invasion, have brought about the collapse of Harappan civilization? The city of Harappa itself and lesser sites in the Indus valley to the north of Mohenjo-daro do not seem to have ever suffered significant flooding. Instead they give the appearance of having been abruptly abandoned, after which they stood empty for centuries. Such a pattern is certainly compatible with the invasion hypothesis. It is also compatible with a situation in which the Harappan state's weakened heartland to the south was unable to send help to the inhabitants of the northern frontier when they were threatened. The people who presented the threat could quite well have been hill raiders rather than Aryan invaders. An archaeological fact must also be taken into account in any effort to reconstruct the Harappan demise: The northern Indus sites show no evidence of a decline in material prosperity before their abandonment but quite the opposite is true of Mohenjo-daro and other southern sites. What does this contrast signify?

The mature phase of Harappan civilization at Mohenjo-daro appears to have degenerated into a well-defined late phase that in turn fades into a squatter phase. Both the materials and style of later artifacts and the quality of later architecture demonstrate a gradual process of degeneration. The traditional Harappan painted pottery of the mature phase, with its intricate black-on-red designs, is replaced in the late phase by plain unpainted ware. In contrast to the typical seals of the mature phase, carved out of soapstone and superbly engraved with animal figures in negative relief, the late-phase seals are not made of soapstone and bear only a few simple geometric designs. The deftly executed and spirited animal figurines of the mature phase are replaced by much cruder effigies. Even the buildings erected during the squatter phase reflect the same degeneration: they are jerry-built and often made of broken or second-hand bricks. These examples of diminishing prosperity in the south, or at least of a debasement in the Harappan civilization's standard of values, suggest an associated breakdown in the efficiency of state administration. Perhaps not only Harappan prosperity but also the Harappan spirit was being mired in an unrelenting sequence of invading water and engulfing silt.

What was the final fate of the Harappans? Findings at more than 80 Harappan sites recently identified in the Gujarat area of India provide a partial answer. A majority of the Indian sites belong to the late phase of Harappan civilization, but the culture had also been present in force in this area during its mature phase. The seaport at Lothal, for example, contains the largest structure of fired brick erected anywhere in the Harappan realm; it is identified by its excavator as a docking basin, ingeniously designed so that the ships within it remained afloat even at low tide.

With the onset of the late phase of Harappan civilization the Gujarat sites present a sad picture of gradual degeneration. Sophisticated Harappan traits are watered down by a mingling with impoverished local cultures until what was once distinctively Harappan is diluted to the point of nonexistence. No urban centers rise along the Gulf of Cambay, no more soapstone seals are carved, no more clay figurines are modeled. Trade with the civilized centers of the Near East, once the *raison d'être* for these Indian coastal ports, comes to a stop. The Harappan script, with its 400 or so still undeciphered symbols, disappears.

These findings are compatible with a hypothesis that envisions the disastrous sequence of floods at Mohenjo-daro and elsewhere in the southern Indus valley as the stimulus that drove the Harappans from the heartland of the far-flung state to take refuge in the Gujarat area. Suddenly crowded with refugees and deprived of support from the once prosperous realm

that had fostered them, the Harappan trading towns on the southern frontier could have done little else than gradually merge with the countryside they had formerly dominated. On the basis of present evidence it seems probable that the Harappans, to borrow a figure of speech from T.S. Eliot, met their end not with an Aryan bang but with an Indus expatriate's whimper.

The Indus Flood Plain and the "Indus" Civilization

H.T. LAMBRICK

In the past few years increasing attention has been attracted to the problem: what caused the apparently sudden decline and extinction of the prehistoric Harappa or "Indus" civilization in its southern sphere, viz., the province of Sind?

One theory ascribes the ruin of Mohenjo-daro to drastic geomorphological occurrences in the Lower Indus plain. The evidence hitherto adduced in support of the alleged occurrence of these particular physical changes seems to me utterly inadequate. Moreover, had such changes taken place as and when suggested, there would inevitably have been consequences other than, or additional to, those assumed by the protagonists and supporters of this theory. The absence of traces left by these *other* consequential processes must cast doubt on the occurrence of those events thought to be reflected in the phenomena actually noticed. The whole conception clearly requires to be reviewed in the light of the ascertained behaviour of the river Indus, and of the physical nature of the Lower Indus plain; and this paper represents an attempt to apply those tests, and to show that other serious objections deserve to be taken into account.

The theory briefly (and, I trust, fairly) stated is as follows: at some period when the Indus civilization was well established and Mohenjo-daro a large and populous city, a violent tectonic disturbance took place in the country many miles down stream. This resulted in a great uplift of the plain, along an axis roughly at right angles to that of the river Indus. It took the form of a swell of ground, many miles broad across its base on the level of the flood-plain, with a height along its crest of more than 100 feet above that of the plain. This swell or bank is assumed to have extended over thirty miles from the rocky rising ground on the western or right-hand limit of the flood-plain to the sand-covered rising ground on its eastern or left-hand limit. The effect of this barrier was to arrest the normal progress of the river Indus to the sea. The flow, held up against it, gradually "ponded" backward up the "valley," forming a very large lake. In due course the rising water level in this lake surrounded, penetrated, and finally submerged the city of Mohenjo-daro. After a period, the length of which may have been of the order of a century, the waters of the lake succeeded in overtopping or breaching the barrier, the river again flowed into the Arabian Sea, and a period of rejuvenation began, at the end of which the Lower Indus flood-plain had (as I understand the argument) resumed its former configuration.

According to one version of the theory, this process of tectonic uplift, arrest of the Indus, creation of the great lake, and submergence of Mohenjo-daro, must have occurred two or three times, at intervals of perhaps as many centuries. In another version, barriers of this nature, productive of like effects, are thought to have been thrown up across the Indus plain at points much nearer the sea than the position of that assumed to have caused the ruining of Mohenjo-daro.

The fundamental idea—of tectonic uplift in the Lower Indus plain causing, or contributing to cause, the destruction of Mohenjo-daro by complete submersion—seems to have been first suggested by the palaeontologist Dr. M.R. Sahni in 1952 (Sahni 1952:153). He developed his views in an article published in 1956 (Sahni 1956). The data on which his theory was based had been observed by him in 1940-41: namely, a thick mass of alluvium containing shells of freshwater snails, lying on Budh-jo-Takar, a flat-topped rocky hill about 24 miles south of Hyderabad,

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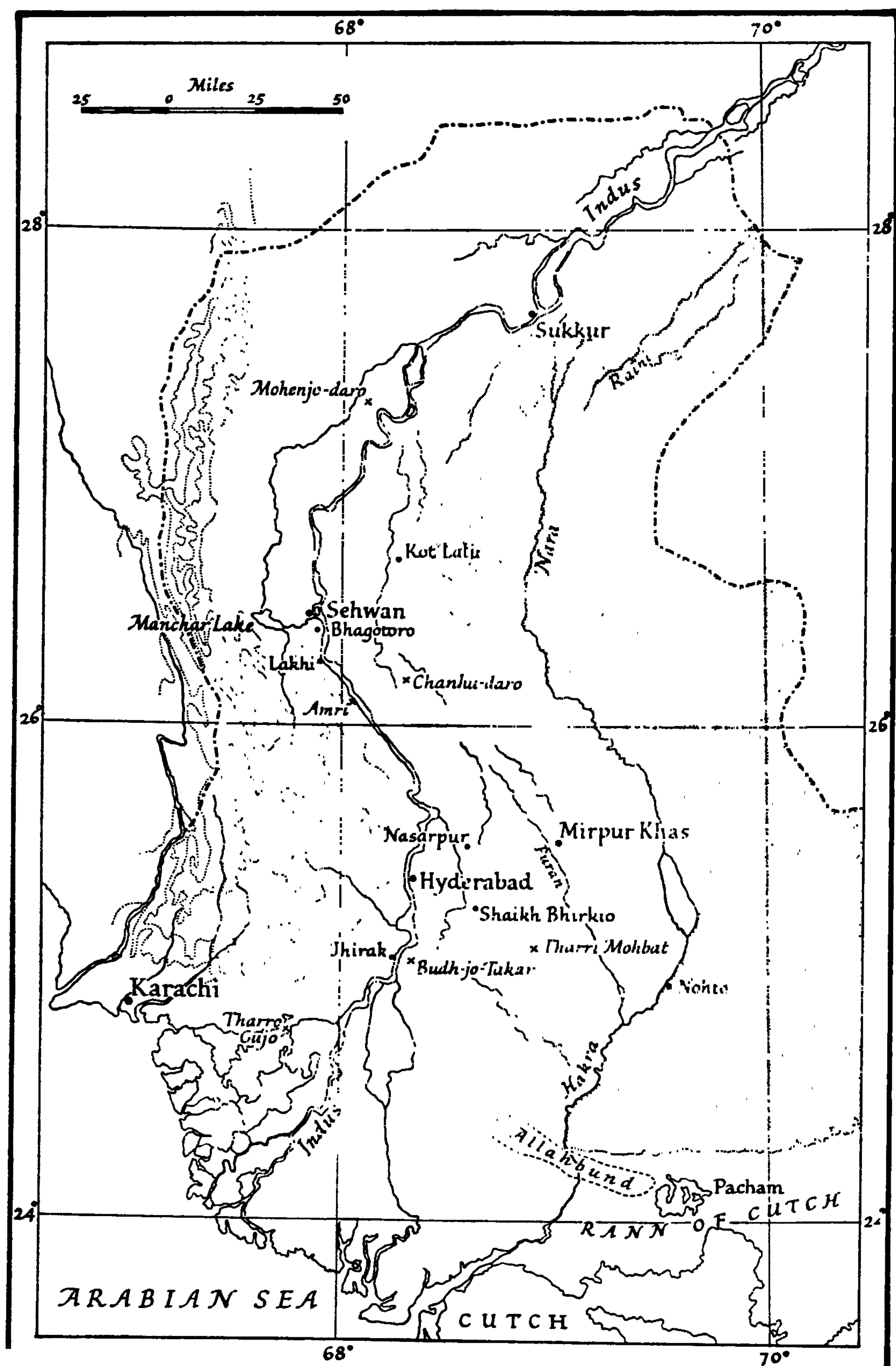


FIGURE 40.1. *Location map of Sind.*

Sind, at a level at least sixty feet above the bed of the Indus flowing near by. Attributing this alluvium to prolonged flooding above this height, Dr. Sahni made mention also, as relevant to his theory of down-stream uplift, of the fact that the great earthquake of 1819 had thrown up a swell of ground many miles in length, though of no great height, across the plain elsewhere in Lower Sind—the Allahbund.

In 1960 other investigators observing geomorphological effects in the neighbouring territory of Makran began to consider the possibility of similar natural changes in Sind having had an influence in the ruining of Mohenjo-daro. Dr. G.F. Dales, an archaeologist, reported signs that the coast of Baluchistan had been "gradually rising for thousands of years at least," and R.L. Raikes, a hydrologist, sketched out a provisional theory, ascribing the end of Mohenjo-daro and other settlements of the Indus civilization in that quarter to the effect of such earth movements. For discussion of the theory reference will be made chiefly to a subsequent article, "The Mohenjo-daro floods," in which Mr. Raikes expounds the theory in greater detail after investigation on the spot (Dales 1962b; Raikes 1964, 1965c).

He would tentatively locate the zone of uplift in the plain opposite Sehwan—"north of Amri and Chanhudaro." He describes in some detail the form and composition of his barrier; the influence of percolation and evaporation in delaying for perhaps a hundred years the attainment of a level in the resulting lake that eventually submerged or silted up Mohenjo-daro; and his conception of the manner in which the *status quo* would have been restored. Then "the previously buried parts of the city re-emerged, enabling the inhabitants to build on the slopes as well as on the tops of the older mounds." He adds, "The possibility must be considered of more than one uplift episode separated by a period or periods of tectonic repose" (Raikes 1965c:201). Dr. Dales, whose views in general coincide with those of Raikes, has also this to say: "Both the multiple layers of silt at Mohenjo-daro and the evidence of multi-level construction suggest that the city was flooded in this prolonged and damaging fashion no less than five times and perhaps more" (Dales 1966a:98).

If this remarkable sequence of events is to be taken as more than imaginative speculation, we obviously require good evidence that Mohenjo-daro was submerged under water or overwhelmed by mud; and that uplift did occur, producing a barrier of the requisite dimensions, in the vicinity of Sehwan. Before proceeding to examine the data presented as evidence, attention is invited to the accompanying map (Fig. 40.2), based on surveys some fifty years old.

This configuration of the Lower Indus flood-plain[1] reflects the characteristic behaviour of a great alluvial river in natural conditions. The Indus has always built up its bed and run along broad "ridges" of its own creation, till it slips off to one or other side and starts the process anew. The modern contours afford clear evidence of the different courses taken by it over a period of several thousand years. The axial gradient of the plain as a whole is almost constant from above Sukkur to the delta. In the few places where the slope corresponding with the existing axis of the river shows a steeper than average fall for a few miles, the change may be attributed to a particularly abrupt avulsion in the distant past from a previous course of very long standing, e.g., a break-out to the right hand from a W to E reach, and consequent adoption of a N to S direction from that point.

Raikes in his article "The Mohenjo-daro floods" gives an account of the data—numerous occurrences of "silty clay" in the ruins of the city at various heights up to twenty-nine feet above the existing flood-plain—which he holds to indicate stillwater flooding up to and above that level. while distinguishing "silty clay" from "clayey silt" he does not define these or other terms he uses—"sterile material," "silty sand," etc. One cannot but feel chary of admitting as evidence of lacustrine deposition observations described in such vague language. I can best explain myself by outlining the physical composition of such materials as I understand it.

The entire alluvial plain of Sind, from its surface down to any stipulated depth, may be regarded as a virtually infinite number of "skins" of Indus silt, each overlying its predecessor. Each such skin represents the deposit of one year's inundation, possibly with some admixture of similar wind-borne material, and may differ in composition from its predecessor. Nor need this predecessor be deposit from the immediately preceding year's inundation, but may date from many years previously. Thus "non-conformity" may exist in a given depth of Sind alluvium just as among composite beds of rock. Variety in composition of Indus silt will accord with the relative speed of movement of the particular flood which deposited it. Thus the coarsest and most angular particles will be shed by the water and will form on the ground over which it flows

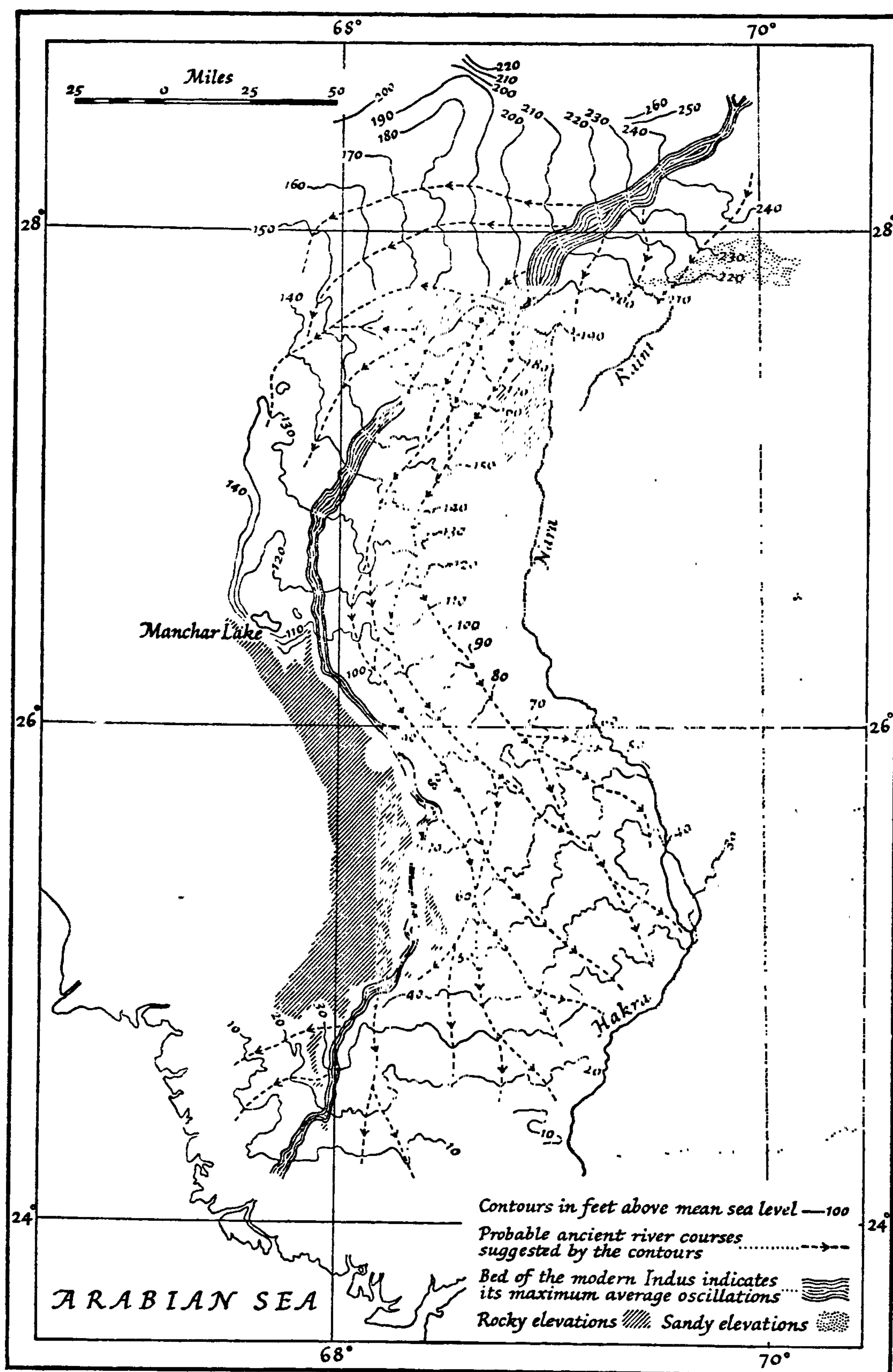


FIGURE 40.2. *Contours of the Lower Indus Plain, with some probable ancient courses and branches of the river which they suggest.*

while that flow is relatively fast. As the speed decreases, lighter particles will be shed, forming a layer of finer texture; and when the water reaches the limit of its flow and becomes stationary, the very lightest grains which have been borne along in suspension while the water was in motion will at last drop. These are so minute that they coagulate readily into an argillaceous substance. "Nonconformity" in alluvium is liable to be found a feature in bore-holes sunk anywhere in the Indus plain, due to the fact that deposition by overspill was not constant in every area, but would vary from year to year according to differences in the inundation discharge, and also in the theatre of overspill, which from the characteristic behaviour of the river was liable to change from year to year. It is also worth remembering that a large proportion of the silt carried by the Indus water in the inundation is eroded from its own banks (which at the cold-weather level stand up like ten- or twelve-foot cliffs on that side to which the main current was setting in the previous flood season) and thus consist of the composite deposits of perhaps several centuries together. Such silt is liable to be a mixture of large, medium and minute particles. If the large predominate to a considerable depth, the inference would be that the material (representing "early" deposition) was built up close to a reach of the river in which it has maintained a virtually unchanged course over a long period.

Reverting to the occurrence of silt high up among the ruins of Mohenjo-daro, one must of course agree with Raikes that this can hardly have been deposited by direct river flood. Again, one recognizes the definite evidence at Mohenjo-daro of periodical rebuilding over former building levels, and may agree that it is reasonable to infer that there were inter.als of time in which the city was deserted and to some extent deteriorated. Raikes and Dr. Dales assume that these intervals were of the order of a century in length and that during them the city was almost entirely submerged, the strata of natural deposits in the ruins representing siltation by Indus water after it had come to a virtual standstill in their postulated lake. But is it necessary to deduce, from the available data, lacustrine deposition, with all the remarkable implications of that hypothesis?

It is necessary to quote Raikes's ideas on these processes. He says, "When siltation was nearly completed there would have been an enormous terrace area extending upstream from the uplift zone"; and again, "Mohenjo-daro and inevitably all other sites in the same general area of the Indus flood-plain were gradually engulfed by mud" (Raikes, 1965c:200, 202). If by this he means that the silted bottom of his lake would have been raised practically parallel with, and eventually not far below, the surface of the water, it must be pointed out that the main theatre of siltation would be over that area in which the inflow of silt-bearing Indus water was slowed up by the resistance of the water already impounded in the lake. The zone of considerable deposit would thus move progressively backward up the "valley" *pari passu* with the extension thither of the surface of the lake. Therefore the natural progress of sedimentation within the lake would seem to be not parallel with the rising surface of its water, but more or less even along its bottom, and thickest along the submerged former course of the river.

How, then, could the siltation of the uplift-dam have reached as high a level as Raikes requires? Why should the rising water bring any silt to seal the upper part of the dam? It would already have shed its burden many miles up stream. If this reasoning is correct, and the dam remained (to quote Raikes) "presenting to such water as was stored above the deposited sediments a very easy percolation passage," the Indus would surely have cut through it long before the surface of the impounded water had risen high enough to submerge Mohenjo-daro. At the outset this obstacle was, *ex hypothesi*, of readily permeable substance; it would in consequence have been unlikely to withstand in the first few years the enormous impact on a relatively narrow front of water arriving at the rate of five hundred thousand cubic feet per second in the flood season. The Allahbund—the swell of ground raised by the earthquake of 1819—was breached by a mere flood of spill water coming down the Nara in 1826; perhaps the first such flood to arrive since the "bund" was raised. Next year Alexander Burnes describes it, "composed of soft clay and covered all over with shells, and has quite the appearance of having been broken through by some torrent" (Burnes 1827, 1834).

If the argument is valid, that the face of Raikes's dam could not have been sealed up to the required hundred-foot level (and more) by lacustrine sedimentation, it follows that the silt observed high up in Mohenjo-daro could not have been deposited by such a process. If, however, the argument be deemed erroneous, and the proposition of a dam sealed by siltation up to the requisite height is acceptable, we are faced by another problem. The greater the degree of

impermeability postulated (and it would have to be impermeable over the whole face of the dam—thirty miles at least, and perhaps very much more) the less likely that all traces of this barrier would have disappeared. The conception of multiple cutting down, etc., is unrealistic We know only too well by practical experience[2] that, though a "bund" may be overtopped or breached in several places by an abnormal flood, the main exit soon locates itself in one of these, resulting in very deep scour through a single breach. And by experience also we know that the substance which would presumably have been the sealing agent for the face of the hypothetical dam, namely the finest colloidal clay such as was at one time deposited by the Indus in an area of slack water in the original canal approach-channel at Sukkur, is particularly resistant to erosion or scouring.[3] At least the butt-ends of such a dam, resting against e.g. the northern end of the Lakhi range and the edge of the Registan beyond Kot Lalu, ought to be visible. What is there at all remarkable at these places? The great fracture in the Lakhi range near Bhagotoro is held to have been caused by the earthquake of 1819 (Blanford 1880). The other local faulting has the same north-south strike as all faulting in the Sind Kohistan. There is the mass of calcareous tufa deposited by the Lakhi sulphur spring, and some isolated sand-hills on the plain towards Kot Lalu. None of these, surely, can be accepted as evidence of existence of the great dam. The geological processes which have affected Makran and Las Bela are irrelevant to the Lower Indus plain; better guidance might be provided by the effects of the Bihar earthquake of 1934 in the Lower Ganges plain.[4] But we require *local* data.

Take next the period or periods of rejuvenation of the flood-plain by the Indus after overcoming the barrier. Raikes says, "This, starting with a system of dendritic gullies, would have reached *fairly quickly* [my italics: H.T.L.] the present regime along the main stem of the system." This conception seems to be far too facile. Moreover it is not a question only of what went on along "the main stem of the system." What was the effect on the flood-plain far away on either side, above the position of the barrier? How was Mr. Raikes's "enormous terrace area" removed? If the silt deposits at Mohenjo-daro 29 feet above the existing plain are the vestiges of its flat surface, his terrace must subsequently have been cut right down to the previous bottom of his lake; for the existing flood-plain, to a depth of over twenty feet, has been built up by ordinary alluvial aggradation since the epoch of Mohenjo-daro. Surely other portions of such a terrace ought to be visible, especially along its western side—if it ever existed.

As to the actual profile of the whole flood-plain of Sind, as recently ascertained, it is difficult to see how this could have been produced except by several thousand years' unimpeded alluvial action of the Indus in natural conditions, i.e., with its characteristic changes of course. The mean axial slope of this plain from Sukkur to sea level is roughly 1 in 8200. The bed gradient of the river is flatter, about 1 in 10500, or six inches' fall per mile. The configuration of the contours indicates that this regime is of very long standing. According to Raikes's diagram included in his article (Raikes 1965c:198), the slope of the existing flood-plain is 1 in 7000. On what basis is this calculated? As to the slope he deduces and extrapolates for the "pre-Harappan flood-plain"—1 in 3500—it is extremely difficult to imagine the Indus accommodating itself to such a relatively steep slope. If the composition of the alluvium was similar to that of today, the river's oscillations in the effort to maintain the regime-slope of 1 in 10,500 would have been catastrophic.

The reasons why Raikes suggests such a slope are evident from his diagram. First, he believes the pre-Harappan mouth of the Indus to have been at or not far below Amri; and secondly, he assumes the river to have flowed in Harappan times, as now, through the "gorge" at Sukkur ("bed-rock control"). Taking up the latter point first—what grounds are there for such an assumption?

The general configuration of the contours considered in relation to the marked link in the axis of the river at this point suggests that the event reflected by that kink—the capture of the Indus by the Sukkur gorge—was of relatively recent occurrence. This conclusion is reinforced by the clear indications a few miles north-west and south-west of Sukkur of a former bed crossed by the present one. Apart from this, not one of the geographies, chronicles or histories dealing with the country makes mention of this passage of the Indus through a rocky gorge earlier than the thirteenth century A.D.

As to the idea of the sea extending nearly up to Amri shortly before Harappan times: the plain opposite this place is now about ninety feet above sea level. If we allow seven inches per century as the average rate of aggradation of the Indus floodplain and delta (Inglis 1949:172), and refrain from mixing in such a con-

jectural factor as some difference between the levels of the sea in Harappan and modern times, we should expect the Harappan coastline of c. 3000 B.C. to approximate to the modern 30-foot contour. That contour lies on the average about sixty miles inland from the sea (corresponding with the river's regime slope of six inches per mile), and not far short of one hundred miles below Amri, measured axially along the plain. But if we postulate a considerably higher sea level in Harappan times so as to extend up to Amri, how are we to explain the presence of the pre-Harappan "Amri" people on the site of Tharro Gujo? The plain adjoining this place is about twenty feet above the existing sea level, and the rocky "inhabited" area only some twenty-five or thirty feet higher—roughly the same elevation as the prehistoric site of Garho Bhiro near Nohto, over one hundred miles to the eastward. The plain opposite Amri stands now at double the height of these places (Cousens 1929:46; Majumdar 1934:20-21).[5]

I have entered into these peripheral matters because they provide additional tests of the validity of the main theory. Mr. Raikes claims that it is "a geologically plausible and hydrologically acceptable interpretation of the flood evidence . . . that fits the known archaeological facts." This begs the question whether the observed data really amount to "flood evidence." But if they are susceptible of a different interpretation it may appear that there is no evidence of flooding beyond that which can be interpreted as the effects of ordinary river-flood; in which case the whole theory must be rejected.

It is incumbent on the critic who views the matter in this light to propound his own interpretation of the data. Accepting that natural Indus silt of some kind is present in the ruins of Mohenjo-daro at various heights up to twenty-nine feet above the existing flood-plain, how did it get there?

The site of Mohenjo-daro, before any excavation had been undertaken, consisted of several large mounds of grey-white earth, with a pinkish tinge from the burnt brick-bats scattered over them or protruding from the surface, among occasional bushes and shrubs (Marshall 1931: pl. 3, opp. p. 10). In other words, it looked just like dozens of other old sites in Sind though on a larger scale than most of them. How were these masses of grey-white earth piled up over ruins of man's habitation and handiwork in the plains of Sind—prehistoric like Chanhudaro; Buddhist like Kahujo-daro near Mirpur Khas; medieval like Tharri Mohbat? (Majumdar 1934: pl. IVb, opp. p. 26; Cousens 1929: pl. XXII, Fig. 1; *J. Sind Hist. Soc.* 1946:62). After allowing a proportion to be the debris of disintegrated mud walls, etc., a vast quantity from some other source must have contributed to produce the characteristic configuration of these mounds. If we ascribe it to lacustrine flood-sedimentation, we have to postulate individual uplift mechanisms at work virtually everywhere in Sind; and are next obliged to ask ourselves, were the similar mounds over ruined habitation sites in Bahawalpur and the south-west Punjab the result of yet other lakes, and further spheres of tectonic uplift? And we must not forget that Dr. Sahni requires uplift of this kind down stream from Budḥ-jo-Takar, to produce the phenomena at that place.

Clearly, some other influence must have been at work; and the obvious one is the wind, periodically whipping sand, silt and dust off the surface of the grey-white alluvial plain, and depositing it in every hollow or interstice among the ruins. Over the centuries rainfall, with further disintegration of the mud buildings, consolidates the mound into the form which is such a familiar feature in the Indus plain. The important fact must be borne in mind that wind-borne silt naturally tends to be composed of the lighter grains; and when these are consolidated by rain, the texture of the resulting "silty clay" (or "clayey silt"?) is liable to resemble very closely the substance resulting from deposition in a lake or by a very slow-moving river flood.

Or take the detached sand-hills standing on the flood-plain of the Indus; those, for instance, about the latitude of the hypothetical barrier. These surely are accumulations of wind-borne silt from the banks and islands of the river exposed at its cold-weather level. Many a village in Sind has been obliterated, in modern times, by air-borne river silt.[6]

In the light of such general observations and of the data which Raikes and Dales give us in regard to the silty deposits high up in the ruins of Mohenjo-daro, it is suggested that these may be due to:

(1) Disintegration of sun-dried brick-work, solid mud plinths, etc., which we know existed among the burnt brick buildings. A certain amount of subsidence of structures of all kinds around the outskirts of the city is likely to have occurred in seasons when a particularly copious inundation temporarily insulated the city; and such movements, with seepage, would tend to affect the stability of buildings further inward.

(2) Consolidation under rain, and pressure by sub-

sequent buildings, of wind-borne silt which, if the climate of Sind in the days of Mohenjo-daro was anything like that of which we had (unpleasant) experience,[7] would have been blown all over the city periodically. Let it be remembered that the burnt bricks, the sun-dried bricks, the man-made consolidated mud fillings, the clay, the "natural" silt whether river-borne, lacustrine or air-borne, the sand-hills—all this material in the plains of Sind was *originally* water-borne, the offspring of the Indus, deposited somewhere. When the mud bricks were disintegrated and the wind-borne silt consolidated, by what criteria could they be distinguished with certainty?

It may fairly be asked, why then did the Mohenjo-daro people allow their city to be cluttered up with wind-blown silt? And what induced them to undertake all this periodical rebuilding, so that the place was raised higher and higher, the topmost structures eventually reaching a level sixty or seventy feet above the then existing flood-plain? It is thought that the city existed, from the first Harappan settlement till its abandonment, for less rather than more than 1000 years. The aggradation seems out of all proportion, even allowing for the recurring subsidence of peripheral buildings whenever the city was insulated by heavy seasonal overspill. Moreover, in view of the drainage system, and ancillary municipal scavenging, the general rise cannot have been due, as at Ur, primarily to constant raising of the street levels. Nor can the mere passage of this span of time account for deterioration of the buildings on successive levels at intervals which, when fitted into the total period, must seem short in comparison with the lasting quality of the burnt-brick masonry. Raikes and Dr. Dales ascribe the deterioration to long, and it seems recurring, periods of total immersion. This explanation seems untenable on other grounds. Is there an alternative?

The concept of Mohenjo-daro flourishing continuously for nearly a thousand years constitutes an exception to what we know to have occurred in every other old town on the Sind flood-plain. Clearly the site must originally have had great natural advantages; the Indus running on an apparently stable course within easy reach, and in the annual inundation fertilizing the adjoining plains with copious overspill, but not tormenting them or invading the city with deep, swift and destructive floods. The wonder would be if such conditions remained constant. It is safe to assume that they did not so remain; that the Indus, though not changing course violently during this long period, did occasionally deny to the vicinity of the city the overspill on which its agriculture mainly depended. We know by experience (in pre-Barrage days) what problems a short-fall in the *abkalani*[8] used to cause, locally if not generally. There are virtually certain to have been many lean years among those thousand. It was for such years that the great granary was provided; but how were the people to obtain subsistence if there were several successive absences of serviceable inundation locally—perhaps for a whole decade? The Indus in natural conditions usually provided overspill *somewhere* along its lower course, and on such occasions the recourse of these thousands of citizens would have been to evacuate Mohenjo-daro temporarily and set up camps in the nearest quarter where favourable conditions for cultivation still obtained. But understanding, as they may be presumed to have understood, the idiosyncrasies of their river, they would be slow to give up hope of a recurrence of favourable conditions near the city. Unless there had been a major change of course, the prospect remained that the Indus would once more show itself benign; and when this occurred they would return to recolonize the city. What, then, were they likely to find? That in their absence silt and sand had blown all over Mohenjo-daro, and was now piled up in houses and streets; that there had been looting and damage done in the deserted city by the subject races of the Indus Empire.[9] Roofs and doors had been carried off, drains had become choked, and rain-water had seeped in where it should not, shrubs seeded from the jungle tracts had begun to grow up in odd corners; hyenas and other animals had been digging and rooting and fouling the place. It was in a sorry state. A large measure of rebuilding would be necessary; so labour is conscripted on a grand scale; masses of mud and mud-brick infilling are introduced and the banked up accumulations of silt smoothed down, to provide a new level. New houses rise on the stubs of former walls, additional courses of brick-work bring the half-buried well-heads up to the new height required, and soon the city's life is back in its old routine. Interruptions of this kind, shorter or longer in duration, may have occurred many times during the span of Mohenjo-daro's existence. So long as the Indus maintained its stable course a few miles away, the original advantageous conditions of the site could be expected to be reproduced, and to last much longer than the interruptions.

Compare with this picture that implicit in the theory

of Raikes and Dr. Dales. Mohenjo-daro has been surrounded by a vast lake, submerged, and finally engulfed in mud—a process lasting at least a century. At an early (?) stage the inhabitants evacuate the city, and live for several generations elsewhere. Eventually Raikes's "enormous terrace area . . . marshy during the annual flood" is somehow transformed back to the ante-diluvian conditions; and the descendants of the flood-refugees return and reoccupy the mud-sodden site. Why? Attracted by what advantages as a place of residence? What could possibly have induced them to act so on a *single* occasion, let alone several times—five according to Dr. Dales! (Dales 1966a:98). Such a concept implies that the people of Mohenjo-daro, pioneers of civilization, were totally lacking in commonsense.

At this point I may outline my own theory of the cause of the *final* ruin and extinction of Mohenjo-daro.[10] I conceive that an avulsion and major change of course by the Indus took place considerably upstream of the city. The new bed being (*ex hypothesi*) lower than the old one and, say, thirty miles away to the eastward, close to the western flank of the Khairpur hills, inundation spill thereafter did not approach within twenty miles of Mohenjo-daro, and the surrounding country, starved of water, immediately began to deteriorate. This theory is not susceptible of proof. But there is strong circumstantial evidence in its favour:

(1) A drastic change of course had become *prima facie* probable from the very fact of the Indus maintaining the same general course past Mohenjo-daro for 900 years or so, with the consequent build-up of its bed.[11]

(2) There is a historical instance, recorded by the geographer Strabo, of this actually occurring in part of the plain dependent on the Indus. That acute observer, Aristobulus, came in 326 B.C. upon a tract which had recently been reduced to ruin in this manner. He explains exactly what the Indus did and what the results were (see Strabo, XV, i, 19).

It has already been mentioned that the general theory under examination was first adumbrated by Dr. M.R. Sahni. The data from which he deduced a flood of "unprecedented magnitude" occurred in a four-foot thickness of "bedded . . . though more or less unconsolidated" alluvium resting on the surface of the rocky hillock of Budh-jo-Takar in Lower Sind, many feet above the bed of the Indus which runs nearby. This alluvium contained the shells of freshwater mollusca. Its thickness suggested to Dr. Sahni that the flooding by which it had been deposited must have been prolonged, and this in turn led him to suppose that such conditions could only have been produced by uplift down stream, due to earth movements.

By a curious chance I happened to examine the hillocks of Budh-jo-Takar in December 1941, a few months after Dr. Sahni's visit, of which I was unaware. My object was archaeological.[12]

There are two distinct hills at this place. On the conical one nearest to the Indus no ancient man-made objects were observed. The other hill is oblong in shape with a flat top, and on the summit, at the end furthest from the river, a patch of earth occurs, where I noticed a number of chert flakes and some painted pottery. Close by there is a regular mound of earth, like a tumulus, with what is evidently a Muslim grave on top. Immediately below, but not apparently an integral part of the tomb, several courses of burnt brick-work like a quadrate plinth were traced; the bricks thin but measuring at least a foot square superficially. Adjoining the mound on the west was a large excavation revealing a depth of earth at this point, on the surface of the hill, of about four feet, and exposing a certain amount of pottery.[13]

In one of the photographs I took at the time of my visit, the view of the earth-face looks as if it was taken from the right of, and nearly at right-angles to, the view appearing as Fig. A in the plate accompanying Dr. Sahni's article (Sahni 1956: pl. 14, opp. p. 106). If this "excavation" was identical with the four-foot depth of earth examined by Dr. Sahni (1956), I have to confess that I did not notice the snail shells he reports. But what I saw suggested to me that here, both in the tumulus and in the thick mass of "excavated" earth below it, was the debris of man-made mud-brick buildings, very probably Buddhist, perhaps a *stupa*, the disintegration of which over twelve or more centuries might have been assisted by the iconoclastic zeal of Muslims, one of whom had elected, as in instances elsewhere in Sind, to be buried on top of the ruins of this "infidel" structure.[14]

As for the presence of snail shells: shells of small mollusca are to be seen resting upon or embedded in Indus alluvium all over the Sind flood-plain; indeed their occurrence is so commonplace that one hardly notices them. Since river snails exist in the Indus, some are bound to be deposited on the plain by its overspill. And if present in the natural alluvium they are liable to be dug up with it, especially by the Odhs wielding their

large *khodars* to hack out building material for mud walls and sun-dried bricks. Snail shells are often to be seen embedded in walls made of these materials.

Thus there are strong grounds for doubting whether the earth on this particular hill represents a natural deposit of alluvium; and before we can accept that explanation of its presence, we should wish to have supporting evidence of similar strata of silt at corresponding levels on other hills in the neighbourhood. It would be strange if the postulated exceptionally high flood left such traces in only one place.[15] Similarly the absence of reports of deposits of "silty clay" in other places within the perimeter of Raikes's lake, at levels corresponding with the 29-foot mark at Mohenjo-daro, must be deemed significant.

The great tectonic uplift-lake-submergence theory of the causes of Mohenjo-daro's decline and fall, thus subjected to critical examination, would appear to rest on entirely inadequate evidence. Meanwhile its promoters and supporters claim, not only that it is "both hydrologically and archaeologically acceptable" but, in so many words, that further researches should be based on it. It is to be hoped that the opposing views, and the grounds on which they are held, presented here *faute de mieux* by a non-scientist, may demonstrate the need for greater objectivity in the search for data and for fresh thinking on what is admittedly a complex and obscure subject.

NOTES

[1]It is convenient to speak of the "valley of the Indus," but one should bear in mind that in Sind this "valley," viewed transversely, is convex instead of concave, so that longitudinally the lowest lines run along the extremities of the flood-plain on either hand, not down its middle.

[2]The writer was actively engaged in measures to avert as far as feasible damage in Upper Sind by the great Indus floods of 1929 and 1930 (before the Sukkur Barrage was in operation), and also witnessed the devastation caused by that of 1942.

[3]Information from S. B. Hickin, Executive Engineer, Barrage Division, 1937-1943.

[4]*Records of the Geological Survey of India*, Vol. 68, No. 2, 1934, pp. 177-239.

[5]See *Journal of the Sind Historical Society*, Vol. 8, 1946, p. 60 for Nohto.

[6]There is an example near Sheikh Bhirkio, about twenty miles south east of Hyderabad. This is close to the well-marked former bed of the Indus—"Phitto"—that runs from a little north of Matiari past Nasarpur and Khesano. Great masses of river silt are piled to a considerable height along this bed as the effect of the strong seasonal wind.

[7]The reference is to the *chaliho*, the forty trying days in May-June; and again to the shorter but very violent dust storms which often usher in a little rainfall later in the hot weather.

[8]The 183-day period which includes the main annual inundation.

[9]I conceive these people—semi-nomadic herdsmen, fishermen and fowlers—to have formed the bulk of the population of the plain, living in brushwood huts or *pish*-mat shelters.

[10]It was developed in more detail in my *Sind: A General Introduction* (Lambrick 1964:81, 84-5, 104).

[11]For this characteristic of alluvial rivers, and particularly the Indus, see Sir Claude Inglis (1949, part I, p. 204).

[12]Viz., to resolve the contradiction between the reports of G.E.L. Carter and N.G.Majumdar on this place; the former having mentioned finding flints "upon two tumuli of brick debris which look like the remains of Buddhist buildings, possibly stupas," while Majumdar declared that nothing of the kind had come to his notice on a visit some years later. In explanation of Majumdar's negative report: his published account mentions his difficulty in crossing the river to Budh-jo-Takar with the evening drawing on. He must have given up his inspection before reaching the eastern end of the summit of the further hill.

[13]My account of this examination of Budh-jo-Takar was included in a paper read to the Sind Historical Society on 27 August 1942, which was published in the Society's *Journal* in October of that year. *J. Sind Hist. Soc.*, Vol. 6, 1942, pp. 110-112.

[14]The concluding paragraph of my article reads, "Mr. Manoo Gidwani, in the last number of this Journal, mentions the legendary association of Budh-jo-Takar with Sultan Bul Bul. May we assume that the tomb on the mound, which is of heroic dimensions, is the Sultan's last resting place?"

[15]Dr. Sahni alludes to Jhirrak in his account, but no specific data appear to have been noticed there. Indeed it is far from clear which data were observed by him and which by Nagappa, or what was the exact location of any of them.

The "Cemetery H" Culture

H.D. SANKALIA

First found by the late Madho Sarup Vats to the south of Mounds AB and J at the foot of Mounds D and E on either side of modern irrigation channel at Harappa, the Cemetery H Culture is the least known of the protohistoric cultures of the Punjab in terms of its extent or distribution and about the people whose cemetery it is supposed to represent.

No structural remains of the Cemetery H people were noticed by either Vats or Wheeler. Wheeler, however, agrees with Vats that there are two strata of burials even though there might not be much chronological gap between the two.

Of these, Stratum II, the lower and earlier one, was described "as earth burials" by Vats; the bodies of the dead were found at an average depth of 6 ft. from the present surface. (How these were placed there, whether by digging a pit, has not been described.) The bodies were kept in an extended position, sometimes with knees slightly bent. The normal orientation of the body was from east to west or north-east to south-west, though there are a few exceptions. In a few cases, only food and drink was provided for the dead. Hence in a few cases no vessels are found; in others they are usually grouped near the head.

A look at these vessels will give an idea of the needs of the dead and their prevailing fashion. The most common ones are: (*i*) water pot (often closed with a flask or handled lid); (*ii*) a coconut-shaped vessel with long, splayed out neck called *kalasa* by Vats; (*iii*) pot-bellied vessel—*ghara*; (*iv*) open-mouthed, round pot with a small standard base; and (*v*) squat, open-mouthed vessel, with a ring base.

Among the uncommon shapes are: (*i*) a bowl with or without a flask inside; and (*ii*) food plate or dish with or without stand.

Of rare occurrence are: (*i*) flasks; (*ii*) saucers; and (*iii*) flat covers.

In Stratum I, pot-burials were found. Since these burials overlay the earth-burials and almost underlay the present surface, many of the pots were found crushed. However, all these pots were invariably large in size. Their height varies from about 13″ to 24″. The three main shapes in these are: (*i*) a round pot, with the lower part plain and roughened by finger decoration or painted and flanged at the neck; (*ii*) an ellipsoid pot with a ring base; and (*iii*) carinated and painted pots with flanged neck. All these burial pots were originally covered with inverted bowls.

Do these types of burials represent the remains of the two different post-Harappan Cultures? What was the relation of the culture(s) to the Harappan on the one hand and the later cultures in India, particularly in the Punjab, on the other? On this problem not much thought seems to have been given, because attention was primarily concentrated on the choronological position.

However, some more light may be thrown on this question, after taking into consideration: (*a*) the nature of the Cemetery H pottery, particularly the paintings on pots of Stratum I; and (*b*) the shapes of pots in Stratum I and II, and their relationship with those found in the Harappan and in the later Indian pottery.

It is conceded by all that though the burials in Stratum I and Stratum II show a marked difference in burial customs and practices, these may still be regarded, as Wheeler said, "as successive phases of the same culture" (1947a:118), because of a community of technique in pottery and patterns on it. He further says that "the difference in the ceramic shapes of the two

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strata of the cemetery is functional rather than cultural, in that large pots of Stratum I were meant to contain the actual burials, whereas Stratum II served only as grave-furniture. The ware in both is burnt deep-red and has a notably bright red slip. The painted designs in jet black are often slightly blurred at the edges as though they had 'run' on a wet ground. The majority of the characteristic patterns such as star, fish, peacock, ox and goat are common to both the strata. Stratum I shows larger groups of these with obviously mythological significance, while on the smaller vessels of Stratum II the designs seem for the most part to be purely decorative but this difference may reasonably be ascribed to difference in size and function." Wheeler thus concluded that "the pottery from Cemetery H is essentially alien in type, technique and decoration to that of the true Harappan Culture. Its distribution is at present unknown but it has been identified also at Lurewala and Ratha Thesi in Bahawalpur State.[1] Its fabric has finer texture and a darker red tone both in the core and in the slip. Its decorative motifs do not include the intersecting circle, scale and other patterns, characteristic of the Harappa Culture. These facts, combined with the structural evidence noted earlier and the thick deposit intervening between Cemeteries R-37 and H, firmly indicate a time interval between the two cultures" (Wheeler 1947a:118-9).

The structures referred to above are indeed fragments of poorly constructed buildings, presumably dwellings with walls sometimes only one brick in thickness. It is indeed surprising as to how these structures are associated with pottery of the "Cemetery H" industry. For, if the occupants of these thinly-built houses were poor, and squatters how could they produce this fine pottery, which is superior in technique and decoration to that of the Harappan? Wheeler once thought (by implication) that the Cemetery H belonged to the Aryans who had destroyed the Harappan culture. His argument is no longer valid since no less than 7 feet of debris intervened between the two deposits and therefore the Aryans could not have destroyed Harappa.

Apart from authorship of the Cemetery H, can anything be said about the people, their racial type and above all their way of life?

Without identifying the Cemetery H people with any particular racial stock, the late Dr. Guha had said that the skulls from Stratum II belonged to a large-headed dolicocephalic type with well-developed supra-orbital ridges and high cranial roof, long face and prominent nose. These features, according to him, showed a continuity of the Indus people of Harappa and Mohenjo-daro. However, in the pot burials of Stratum I, Ghua had observed an element of small, low-headed people, and this, Vats thought, indicated "a definite admixture" which might be due to racial or cultural upheaval brought about by the immigration of a foreign people into this district of the Punjab (Vats 1940:234).

So much about the racial types. If, however, we turn for a moment to the pottery found in the two strata, it shows three things:

First, a workmanship finer than that witnessed in the Harappan, and this in all the aspects of the potter's art: (*a*) preparation of the clay, (*b*) potting, and (*c*) baking. The Harappan was already known for its thick, well-baked pottery. In addition to all this, the Cemetery H pottery is painted. And there are not just haphazard, unconnected patterns and designs, geometrical and natural. Paintings depicted on the large pots of Stratum I indeed relate a story, may it be mythological. But these are our earliest full-fledged paintings, important both from the point of art and the story they relate. Whether the paintings were done by the potter's wife (whose work this is usually believed to be) or the potter himself or someone else, the whole story must have been first sketched out and then carefully transferred on the broad shoulder of the pot. And the artist knew how much space each actor in the story was to be accorded, and accordingly drew it or him, small or large, full or in outline. The exact interpretation of the story or stories these paintings relate, will remain problematical until some more data, and in particular writing (which can be read), of the Cemetery H Culture is found. Vats very hesitatingly has drawn our attention to certain passages in the tenth *mandala* of the *Rigveda*. The elaborate painting on a burial pot was meant to portray what was believed to be a dead person (Fig. 41.1). The hound on the left hand scene, may, like the two hounds of Yama, be the hounds of Hades and that the right hand scene, where the cattle are now decked with trident, crests, depicts them in "the Abode of Bliss" after they have passed through the intermediate Hades. The goat between the two scenes was a sort of "path-finder," and a deified intermediary, an inference which may be drawn both from its large size and the crests on its horns (Vats 1940:238).

However, Vats was careful enough to observe that though the paintings recalled some of the rites, rituals

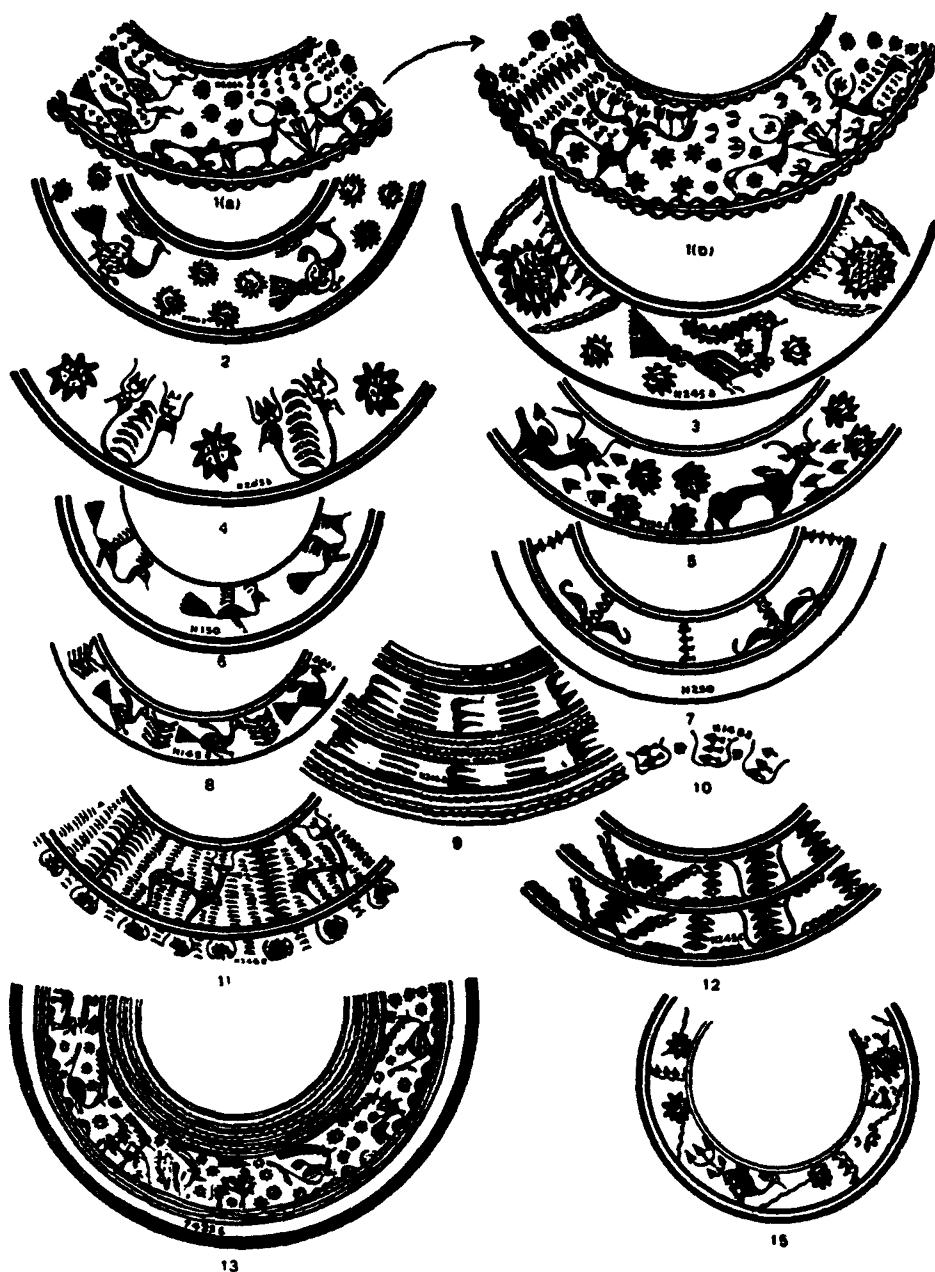


FIGURE 41.1 *Painted panels of mythological scenes depicted on burial jars from Cemetery H (after Vats 1940).*

and beliefs contained in hymns 4, 16, 18 of the tenth *mandala* of the *Rigveda*, still there was one vital difference. The *Rigveda* describes cremation rites, whereas these paintings are connected with post-exposure fractional burials.

At the moment one cannot press this parallelism between the two beliefs any further, but they are pregnant with great possibilities, which are briefly discussed here.

If in the future it can be proved that the Cemetery H belongs to Aryans or the people described in the *Rigveda*, then it might be said that they had some hand in destroying the Harappan civilization, for, as Wheeler has said, these are the only people who are

known to succeed immediately the Harappans. Secondly, whether they can be accused of this great event or not, their pottery was finer than that of the Harappans, and thirdly, their potters could relate the story of the dead so well and in such a short space.

Though we are not sure of the identity of the Cemetery H people, as we are not of the Harappans and many others, still we may reasonably infer about their way of life from the shapes of their pottery found in the two strata of the Cemetery. For this we shall rely primarily on the work of Vats, which was more extensive and whose report on the collections is also detailed. Fortunately, this is confirmed by the small collection of Wheeler.

Vats has classified and illustrated the large collections into 13 groups (A to M), and each of these groups has smaller groups according to size and shape. Thus, no less than 27 forms of pottery are illustrated. These include:

(1) Small and large ellipsoid jars, each having a flange, to receive a large, deep lid with or without a handle (A 1.8).

(2) Bowls of three types (B 9-11).

(3) Small- and medium-sized round jars with fingertip decoration on the lower part and sometimes covered with a small painted *lota* with a ring base (C 12-14).

(4) Medium-sized carinated pots having a ringed base and a broad mouth, with a flange to receive a large and deep lid with a handle (D 15-16).

(5) Water pots with a globular body, long flaring neck (E 18 and 20).

(6) Dishes on squat stand (called food plates by Vats) (F 21-22).

(7) Vessels with elongated body, at times egg shaped, with a flaring long neck, and ring-base. A few of these are painted with a concentric circle pattern on the belly (G 1-10).

(8) *Ghatas* or vessels with a broad mouth, round body, and ring base, or simple and painting on the shoulder, "water pots" of Vats (H 11-12; 16-17).

(9) *Lotas* or round vases, some painted (I 13-15).

(10-12) This group represents three different types of vessels but have one thing in common and that is acute carination in the belly of the vessel.

(13-15) Saucers or dishes with outgoing sides (K 21-23).

(16) High sided bowl with carination at the base and ring below it (K-24).

(17) Thick-sided dish with beaded rim (L 25).

(18) Flat covers (M 26-27).

These 27 vessels would supply all the essential needs of any household with regard to storing of grain, water and other provisions, as well as eating and drinking.

However, certain forms are quite rare or unique, for instance, (Type F) squat food plates or dishes-on-stand and (Type G) vessels with elongated body and flaring neck and a ring base. In fact, the ring base, dishes-on-stand and such other distinctive features are almost absent in Indian pottery from the early Historic period and later (Gordon 1958a:84). These characterize the prehistoric pottery of Western Asia as already noted by Vats, and occur in the Harappan and the chalcolithic pottery of western and central India. Thus, genetically the Cemetery H pottery is foreign-oriented. Even among its plain dishes, saucers and flat covers, we miss the familiar Indian *thali* (a flat dish with rounded corners and straight or inturned edge). These are even absent in the Harappan, but seem to appear for the first time at Navdatoli, and later in the Painted Grey Ware. Likewise, vessels with conoid and ring base, and large lids with a handhold figure in the Harappan but are rare or absent later. The same may be said about its dishes-on-stand. All those from Harappans whether having a bowl or saucer-like upper part, have a long stem whereas those from the Cemetery H have a short stem with mouldings. Thus if any relation has to be inferred between the Cemetery H people and their way of life, it will have to be with the later Indian people and their lifeways.

And for this comparison there is no better repertory of Harappan pottery than the Cemetery R-37 at Harappa. Here one finds a bowl or dish-on-stand with mouldings (Fig. 24, 3), large lid with a prominent handhold (Fig. 23, XXXIX and Fig. 20), saucers and dishes (Fig. 19), and slender vases or flasks with elongated concavo-convex sides, and a small base, but not as narrow in the Cemetery H (Fig. 18, XXVIII and XXNIII d). However, these seem to be the direct ancestors of the Cemetery H flasks. (All in Wheeler 1947a.)

In short, in the present state of our knowledge, the Cemetery H people culturally and racially do not seem to be far different from the Harappans.

It is impossible that the makers of this beautiful painted pottery should be confined to the area around Harappa. At least the whole of the Punjab, northern

Rajasthan and probably Sind should have been the home of the Cemetery H people.

Some of the points here raised for the first time were anticipated by Bridget and Raymond Allchin (1968:149), but not taken to their logical conclusion. They seem to agree with Vats that the Cemetery H culture was the final stage of the Harappan and continuous with it; but that it must indicate the presence of foreign conquerors or immigrants. This might indicate a sort of cultural fusion which may be represented by the "Cemetery H" culture itself. They further thought that the pottery showed some affinities with wares from Tepe Giyan (Strata II-III) and Djamshidi II and Susa D, all dated to *circa* 1500 B.C.

NOTE

[1]Gordon (1958a:25) thinks that sherds similar to those of Cemetery H occur at Bara and Rupar. This appears to be correct as far as the design motifs such as the arrow-head, 8-rayed star and fish are concerned. But the fabric of these should be examined.

Part X The Radiocarbon Chronology

Editor's Introduction

Three elements of this part form a comprehensive view of the radio-carbon chronology of the Indus Civilization. The list of radiocarbon dates with correction factors forms the substance of the data. The paper "Radio-carbon Dates and Reality" will allow anyone to use the most up-to-date set of calibration figures for their own adjustments of the dates. George Dales' paper is a general discussion of some of the implications of the adjusted radiocarbon time-scale.

Dales highlights two important historical problems. First, in the Early Harappan period there is fairly extensive evidence for contact and trade between the northern areas of the greater Indus Valley, including Baluchistan, and the region from Seistan to Central Asia. At about the time the Urban Harappan style emerges these contacts are terminated and signs of contact shift to southern Iran and possibly Mesopotamia. Second, it has been widely noted that a majority of the Harappan material ties to Mesopotamia fall within the Akkadian period centering on 2350 B.C. (Wheeler 1968:110-20). An examination of the radiocarbon dates from Harappan sites indicates that with rare exceptions the Urban Harappan seems to bridge the time between 2100 and 1700 B.C. We are thus left with a disparity of something in the vicinity of 250 years between the two calendars. The dendro-corrected calendar, however, brings the radiocarbon chronology into alignment with the material evidence and removes this inconsistency.

Archaeological and Radiocarbon Chronologies for Protohistoric South Asia

GEORGE F. DALES

The early chronology of the Indus Valley and the eastern half of the Iranian plateau has been fraught with problems since the discovery of the Indus civilization in the 1920s. Until recently the tendency has been to derive the initial cultural impetus for the rise of civilization in South Asia from the Near East—mainly Mesopotamia. Mesopotamian chronology and dating were also imposed on the cultural development of South Asia and the intervening regions on the Iranian plateau. But by now the increasing quantity and quality of archaeological investigations in the East is making it possible to develop the chronologies of these regions on their own internal evidence (Allchin and Allchin 1968; Casal 1969; de Cardi 1965; Wheeler 1968). Also, the use of radiocarbon dating has added an important new dimension to the solution of the chronological problems (Agrawal 1964, 1966; Dales 1965a, 1971).

This concern with chronology and dating is of more than academic importance at this stage of our knowledge. For example, as emphasized by Colin Renfrew (1970), new and more accurate cultural and historical configurations are possible to discern as chronologies and temporal interrelationships are refined. This is especially vital here as we learn more about the extent and complexity of "international" trade and other inter-cultural contacts.

The purpose of this paper is to review the current status of the chronology and dating of the fourth and third millennia in the eastern Iranian plateau region and the greater Indus Valley. This will be done first from the standpoint of traditional comparative archaeology (Fig. 42.1). Secondly, the pertinent radiocarbon dates are tabulated and discussed (Fig. 42.2). Thirdly, the new correction factors proposed by the physicists for radiocarbon dates are tabulated and evaluated in relation to the first two mentioned types of chronological frameworks. Although it is not my intention to delve deeply into matters of interpretation and reconstruction, a few patterns emerge from the raw chronological data that are suggestive and of potential historical and cultural importance. Finally, the role of Seistan, that once rich and productive region that now straddles the desolate border between south-western Afghanistan and eastern Iran, is discussed.

The Archaeological Chronology

Fig. 42.1 summarizes the primary correlations between the early sites of the greater Indus Valley, Baluchistan, southern Afghanistan, Seistan, and Turkmenistan. There are, and always will be, differences of opinion about specific correlations, but in general I believe the figure has practical validity. The correlations suffer badly from the fact that archaeology in this part of the world is still ceramic typology bound and only a few sites have received more than preliminary trenching. Two of the sites on the chart, Gumla and Jalilpur, have been too recently excavated to be generally known through published reports. Gumla, in the Gomal Valley on northern Baluchistan, was excavated by Professor A.H. Dani of Peshawar University. Jalilpur, in Multan District, Pakistan, was excavated during the summer of 1971 by Dr. Rafique Mughal of the Pakistan Department of Archaeology. Both of these sites are extremely important for the new information they are shedding on contacts between the Indus region, Afghanistan, and Turkmenistan. Radiocarbon samples from both sites are being tested.

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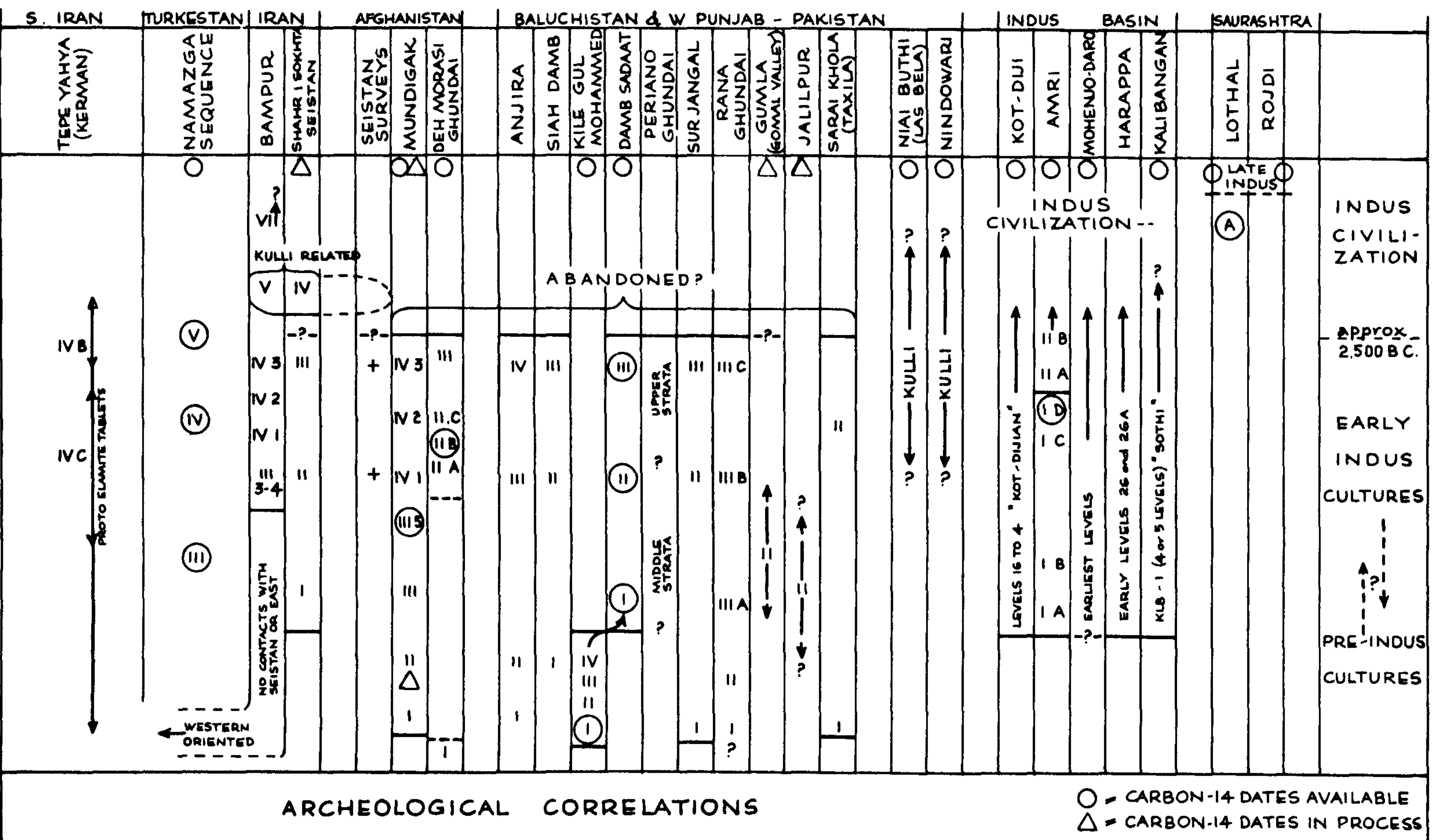


FIGURE 42.1 *Archaeological chronology.*

A word concerning terminology is necessary. South Asia's earliest civilization, centred in the Indus Valley, is known to us only through its mute archaeological remains. Its writing system has yet to be convincingly deciphered. Thus the civilization has been called either Harappan (after the modern name of one of its major cities) or the Indus civilization. Both names have drawbacks. To use a single site name focuses undue attention on a settlement of unknown significance and function in the total civilization. The discovery of impressive ancient settlements of this culture outside the Indus Valley—especially in Saurashtra—has put into question the propriety of using the label Indus civilization. Even more troublesome is what to designate those occupation levels and sites having cultural materials that are stratigraphically and typologically earlier than what is generally accepted as the mature manifestation of this civilization. The term "pre-Indus" begs the question and conveys the impression that we know more about the origin of the civilization than we do. Recently, a detailed review of all the evidence from the early sites of the Indus Valley and northern Baluchistan has produced convincing criteria for recognizing what may have been the early formative phases of the civilization (Mughal 1970). Dr. Mughal has proposed the terminological scheme "Mature Harappan," "Early Harappan," "pre-Early Harappan." But one can object that his use of "Early Harappan" implies that all the early settlements in the greater Indus Valley and northern Baluchistan dating from about 3000 B.C. to 2400 B.C. were participating in the general development towards the mature urbanized civilization that is so dramatically exemplified by the city-sites such as Mohenjo-daro, Harappa, and Kalibangan. This terminology question was informally discussed among several members of the 1971 Cambridge conference (R. Allchin, B.K. Thapar, J-M. Casal, B. de Cardi, and myself). Final agreement was not reached but there was general satisfaction expressed for a scheme such as this for the greater Indus Valley:

Indus Civilization: Designates the "mature" maximal stage of the culture as exemplified by the major city-sites and described most eloquently by Sir Mortimer wheeler (1966 and 1968). Despite the objection raised about the name "Indus," it is thought best to retain it because of its wide and popular usage.

Early Indus Cultures: Designates those various cultural deposits either stratified directly beneath remains of the Indus civilization or found at smaller sites otherwise dated before the period of the Indus civilization. Some of these—e.g. the early levels at Kot-Diji (Khan 1965; Mughal 1970), the "Sothi" levels at Kalibangan[1] and the "pre-defence" material at Harappa (Wheeler 1947a) qualify for Mughal's definition of an early formative phase of the Indus civilization. On the other hand, the early levels at sites such as Amri (Casal 1964a) do not contain material that anticipates that of the Indus civilization and should be classified as belonging to an early Indus culture of still undetermined affinity. Even those sites that belong to the early formative phase of Indus civilization should, on present evidence, be regarded as relatively independent from one another. They shared certain common traits, styles and techniques that eventually coalesced to form the mature Indus civilization, but originally these sites had other traits that were peculiar in their own micro-region.

Pre-Indus Cultures: Designates those sites or early levels of sites that are temporally much earlier than the first discernible settlements that developed into the Indus civilization. The dividing line—temporally and artifactually—has yet to be determined through extensive and intensive new excavations in Baluchistan and the greater Indus Valley.

This tripartite classification is admittedly gross. New and more extensive excavations will hopefully increase our knowledge not just of ceramics and artifacts, but of settlements, settlement patterns, and environmental details that will allow us to understand better the processes involved in the development of South Asia's earliest civilization. The terminology just discussed does not apply to those regions west of the present western border of Pakistan. For southern Afghanistan and eastern Iran it is still necessary to identify cultural groups by site names (Dales 1971). If the present work of the Italian archaeologists in Iranian Seistan continues (Tosi 1968, 1969, 1970a, 1970b) we should soon be able to develop a more meaningful classification of the occupational sequence. The importance of the Seistan region cannot be overemphasized. Historically, and now archaeologically, it is evident that Seistan was at the hub of "international" trade and other contacts between South Asia, Central Asia, and the Near East as early as the middle of the fourth millennium B.C. Although Seistan did not apparently have first-hand contacts with the Indus

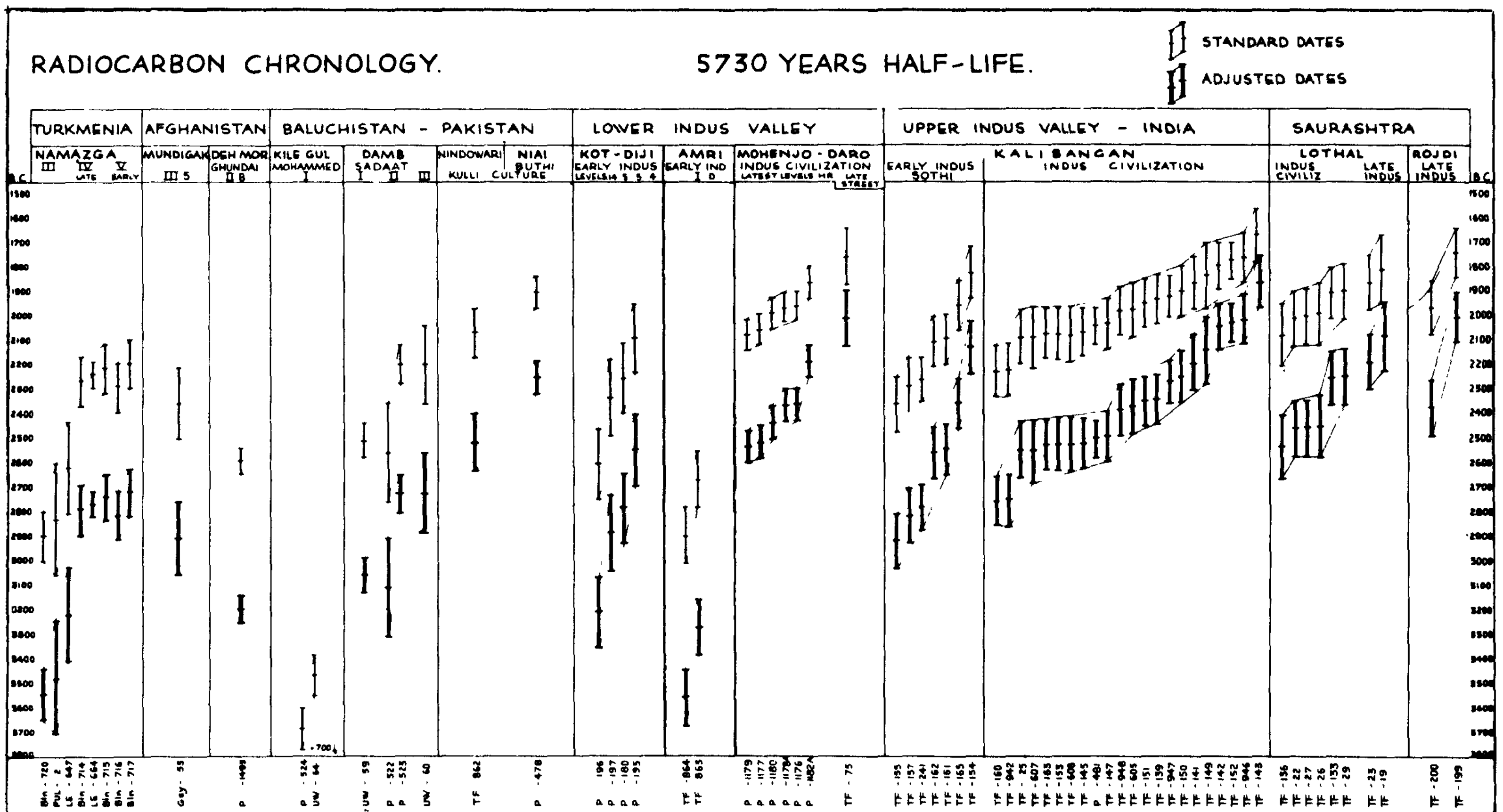


FIGURE 42.2. *Radiocarbon chronology.*

civilization, it did have strong contacts with some of the Early Indus Cultures and areas such as southeastern Afghanistan (Mundigak, Deh Morasi Ghundai) and Turkmenistan. Thus a stronger chronological framework for Seistan is essential to that of the entire region covered in this paper.

The Radiocarbon Chronology (Fig. 42.2)

The shortcomings and abuses of C14 dating are becoming proverbial in archaeological literature. However, from the standpoint of the South Asian evidence, the main thing to be discouraged about is the lack of enough dates and the dismal prospects of obtaining many in the near future. What few dates are available must be carefully and consistently analysed. Here I have converted all the published dates to the 5,730 years half-life as is the standard practice at the University of Pennsylvania Radiocarbon Laboratory. Certain dates have been excluded, namely, those derived from the old solid-carbon samples, dates with inordinately large tolerances, and dates that are logically inconsistent. The narrow vertical bars in the upper part of Fig. 42.2 show the maximum range of each date as published in *Radiocarbon*. Dates belonging to the same phase or period of a site are enclosed by solid lines so as to emphasize the total block of time represented by the dates rather than the limits of any single date. The laboratory numbers are given for each date so that details of their publication can be found in *Radiocarbon*. On Fig. 42.1, circles are placed around those site periods or levels that have provided radiocarbon dates. The triangle signs indicate those sites that have provided new carbon samples that are currently in the process of being dated.

Comparison of the relative temporal position of the blocks on the radiocarbon chart (Fig. 42.1) and the corresponding cultural phases or levels on the archaeological chart (Fig. 42.2) shows a generally consistent correspondence. There are some anomalies, e.g. the radiocarbon date for Deh Morasi Ghundai seems too early and that for Mundigak III, 5 too late, but single samples are dangerous to rely on at any rate. The Amri dates—only two—also appear to be too early. The Tepe Yahya dates so far published are too inconsistent and out of line with accepted archaeological parallels to be meaningful (Lamberg-Karlovsky 1970, 1971). New dates are presently being calculated from this very important site in southern Iran that may help clarify some of the chronological relationships between southern Iran and Seistan. Finally, a puzzling situation is presented by the almost total overlap of the radiocarbon dates from the "Sothi" and Indus civilization levels at Kalibangan. Preliminary reports on the excavations give the impression that the "Sothi" phase proceeds the mature Indus civilization occupation and has been regarded by Mughal (1970) as belonging to the general early formative phase of Indus civilization.

New Adjustments for Radiocarbon Dates

As if many archaeologists were not already confused and discouraged by the apparent unreliability of radiocarbon dating, the physicists are now proposing major adjustments to bring radiocarbon dates more in line with historical reality (Michael and Ralph 1970 and 1971; Renfrew 1970). Correction factors have been determined from inconsistencies between radiocarbon and denrochronological chronologies. For the B.C. era, the correction factors that should be added to the standard radiocarbon dates grow progressively larger as you go earlier in time—from plus fifty years at 300 B.C. to plus 650 years at 2700 B.C. The most recent calculations for the time period covered in this paper are as follows (Michael and Ralph 1971).[2]

Time period represented by radiocarbon dates (5,730 half-life)	Average correction to be added
1551 B.C.—1750 B.C.	200 years
1751 B.C.—1900 B.C.	300 years
1901 B.C.—2050 B.C.	400 years
2051 B.C.—2225 B.C.	500 years
2226 B.C.—2450 B.C.	550 years
2451 B.C.—2650 B.C.	550 years
2651 B.C.—2850 B.C.	650 years

Thus, for example, a published radiocarbon date of 1950 B.C. should be corrected to 2350 B.C.

These correction factors have been added to each date in Fig. 42.2 and are represented by the heavier vertical bars on the lower part of the figure. Note that because of the progressive increase in the size of the correction factors, the blocks of dates for each site phase or level are elongated and encompass a longer time span than do the blocks for the unadjusted dates. At first glance this appears to make the dating for any particular phase or period less precise than the unadjusted dates seem to indicate. Actually, however,

given the innate variables of any radiocarbon determination, the broader range suggested by the adjusted dates may be more realistic. This emphasizes even more strongly the desirability of treating C14 dates in blocks and units with rather large overlapping tolerance levels rather than placing much reliability on single isolated samples.

Comparison of the adjusted dates with the archaeological correlations presents us with a chronological structure that seems to fit much better than that using the unadjusted dates. For example, using the adjusted dates (with the 5,730 half-life) for the Early Indus Cultures, we get close agreement with the archaeological parallels that link phases such as Mundigak IV, Bampur IV, Namazga IV, Susa D (south-western Iran) and the Mesopotamian Early Dynastic period. Similarly, the adjusted dates for the Indus Civilization bring the Indus chronology directly in line with the historical chronology of Mesopotamia. The adjusted Indus dates compare closely with the accepted historical dates for the Mesopotamian Old Akkadian and Ur III periods. These were the periods of considerable seafaring activities by the Mesopotamians and of the presence of Indus-type artifacts in southern Mesopotamia. The adjusted dates also allow for a late slowing down and eventual disappearance of the classic Indus civilization by about 1900 B.C. This coincides favourably with the latter part of the Mesopotamian Isin-Larsa period and the cessation of Mesopotamian seafaring activities.

The adjusted dates also correlate better with the archaeological parallels between the Early Indus Cultures, Afghanistan, Seistan, and Turkmenistan and add support to the Russians' comparative dating of their Namazga III period (Mesopotamian Ubeid-Uruk periods) and Namazga IV (Jamdat Nasr) (Dyson 1968; Masson 1961 and 1968; Mellaart 1967; Frumkin 1970). The importance of the new radiocarbon dates from Shahr-i Sokhta, Mundigak II (collected by G. Dales 1969),[3] Gumla, Jalilpur cannot be overstressed. They are expected to strengthen the framework of South Asian chronology so that it can be brought to bear on some of the unresolved problems of Iranian chronology and inter-cultural contacts.

Some Historical Patterns

Although the chronological framework is still very slight, a few patterns are beginning to emerge from a study of the archaeological parallels and the radiocarbon charts that go beyond the mundane timetables.

First, the archaeological parallels, reinforced by the adjusted radiocarbon dates, suggest that for several centuries before 3000 B.C. there were strong overland cultural and economic contacts ranging from Turkmenistan (Namazga III) through Seistan (Shahr-i Sokhta I), south-eastern Afghanistan (Mundigak III), through northern Baluchistan and into the sphere of the Early Indus Cultures of the middle and upper Indus basin (Dales 1971).

The archaeological contacts are quite convincing, including not merely ceramic analogies but virtually identical female figurines, art motifs, and especially significant, the widespread presence of lapis lazuli. Workshops, with raw and finished lapis beads are reported at Shahr-i Sokhta (Tosi 1970a) and large quantities of lapis have recently been discovered at the Early Indus site of Jalilpur (Mughal, personal communication). During this period, Seistan and the lower Helmand valley provided the south-western limit of this cultural sphere. There is no evidence of contact between Seistan and Bampur or Tepe Yahya in southern Iran. Bampur's cultural gaze was cast westward, past Yahya to as far as southern Mesopotamia (de Cardi 1970).

However, it should be noted that the Period I levels of Shahr-i Sokhta had not yet been published when Miss de Cardi made the above suggestions. Now with the publication of the second season's work (Tosi 1969), she does see several specific parallels in the painted designs between Shahr-i Sokhta I and her Bampur I and II. Also, there was the common tendency at both sites to apply painted decorations to both the interior and exterior of the rims. The cultural significance of these few parallels, if any, remains to be determined.

Then, at approximately 3000 B.C., we see the first evidence of contact between Bampur (III, 3-4)—with its southern Iranian culture—and Seistan. This heralds the start of dramatic changes in "international" contacts and spheres of contact and influences that culminate by approximately 2500 B.C. in the following new patterns:

(*a*) Archaeological evidence for contacts between Turkmenistan and the greater Indus region cease and we see the independent development of urban civilization in each region.

(*b*) The reason for this break in contact are unknown but a look at Fig. 42.1 shows that some serious disruption had apparently taken place along the land

routes between Turkmenistan and the Indus. Every site, from Mundigak through northern Baluchistan, was abondoned at this time (Dales 1965a, 1971).

(*c*) The result of this unexplained abandonment of the northern land routes is a dramatic shift in the avenues of contact to the south, into southern Baluchistan and along the Indian Ocean Coast. This is the period of the "mature" Indus civilization in the east and of the apparently flourishing seafaring activities of the Mesopotamians in the West (Akkadian and Ur III periods) (Bibby 1969; Dales 1968c; Oppenheim 1954). Contacts between Bampur, southern Iran, and the Persian Gulf were also maximized during this period. Seistan's role changed. Its relations with Mundigak and northern Baluchistan had ceased, but its contacts with Turkmenistan and southern Iran continued. The lapis trade continued but only with the Near East. Very little lapis is found in Indus civilization contexts. Seistan may well have been a major source of raw materials for the Near East. Large quantities of alabaster and other stone are present, both in the raw state and in the form of bowls and other objects. Copper smelting was carried out on a vast scale as is witnessed by the miles of slag and furnaces in the Gardan reg part of southern Afghan Seistan (Dales and Flam 1969; Fairservis 1961a). These areas and materials will receive intensive study during the University of Pennsylvania's 1971-2 project in Afghan Seistan.

Summary

The use of the adjusted radiocarbon dates fits best with the comparative archaeological chronology for South Asia and the eastern Iranian plateau. It also fits well with the archaeological evidence for contacts between the Indus and Mesopotamia. The current field work in Seistan (especially at Shahr-i Sokhta) and in the northern Indus region (Gumla and Jalilpur) is essential to both chronological and dating problems and to the further documentation of cultural and economic contacts between South Asia, Central Asia, and the Near East.

NOTES

[1]The work at Kalibangan and the Sothi complex has been reported annually in *Indian Archaeology. A Review* from 1960-61 to 1964-65. See full citations under ARCHAEOLOGICAL SURVEY OF INDIA in the Bibliography.

[2]Editor's note: Full correction factors appeared after Professor Dales had published this paper, and will be found in the next article. The factors used by Dales are fully valid as they are, however the closer calibration will add accuracy to individual dates.

[3]Three Mundigak dates received while this paper was in press are: Period I, 2-3:3145 ± 110 B.C. (TF-1129); Period I, 5:2755 ± 105 B.C. (TF-1131); Period II, 1, or I, 5(?):2995 ± 105 B.C. (TF-1132). 5730 half-life; on wood charcoal from Mound A.

Radiocarbon Dates and Reality

ELIZABETH K. RALPH, H.N. MICHAEL and M. HAN

INTRODUCTION

The technique of radiocarbon (C^{14}) dating is almost twenty-five years old. In the beginning there was some reluctance to accept C-14 dates; then, there was general reliance upon them. However, we have now learned that one of the basic assumptions of the method—namely, the constancy of the atmospheric inventory of $C^{14}O_2$—is not strictly valid. For various reasons (Olsson 1970), it has fluctuated during past times. Therefore, for most periods, corrections must be applied to radiocarbon dates to adjust them to true ages. The corrections, presented either in the form of a calibration curve or in tables, have been derived from the C^{14} dating of tree-ring dated samples.

Dendrochronology, by means of the tedious process of cross-dating, has provided samples of known age, accurate to within a year. For the correction of C^{14} dates, the first prerequisite was a series of long-lived trees. It has been found that only the outer growth ring of a tree is in equilibrium with the atmosphere and that the C^{14} content of the adjacent inner ring is one year old and so on until one reaches the maximum age or "pith" of the tree. These long-lived series were first provided by the sequoias (*S. gigantea*) which afforded a scale reaching 3100 years back in time (Douglass 1919). More recently, starting in 1954, Schulman (1956) and then Ferguson (1970, 1972) working with bristlecone pines (*P. aristata*) have succeeded in extending the range of known-age trees to approximately 8000 years before the present. However, because of the paucity of wood of this extreme age, precisely dated samples reaching to only 7350 years before present have so far been available for radiocarbon dating.

For the past 15 years, three laboratories have been obtaining C^{14} dates from samples (spanning about 10 years each) of these long-lived dendro-dated trees. The three laboratories are at the University of Arizona in Tucson, the University of California at San Diego in La Jolla, and the University of Pennsylvania in Philadelphia. All three presented results individually, in one form or another at the Twelfth Nobel Symposium held in Uppsala, Sweden in 1969 (Damon 1970; Suess 1970; Michael and Ralph 1970). At Uppsala, it was clear that the C^{14} dates of all three laboratories were in excellent agreement on the *average*, but that there were differences between individual or small groups of dates. (See Olsson 1970:110, 597, 615-618, 619-624, and separate plates I and II.)

At the more recent international C^{14} conference in Lower Hutt, New Zealand, in October 1972, it was anticipated that after three more years spent in obtaining greater numbers of C^{14} dates, from precisely dated wood samples, better agreement could be achieved. With this in mind, the laboratories of Arizona (Damon, Long, and Wallick 1972) and Pennsylvania (Michael and Ralph 1972) presented separately composite plots of all C^{14} dates obtained by all three laboratories, with the Arizona laboratory adding a few dates determined by Stuiver some time previously (Stuiver 1969). La Jolla presented its 1969 (Uppsala) "calibration curve" derived from La Jolla dates only and containing many doubtful short-term deviations based on very few dates. Again, there was good agreement on the *average*, but no decision was reached as to which calibration method best expressed "true" calendric dates.

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Methods Used

Before entering into the discussion that follows it should be established that all of the radiocarbon dates used in this study have been calculated with the 5730-year half-life. The dates published in *Radiocarbon* are based on the 5568-year half-life and may be easily converted to the 5730-year half-life by multiplying the B.P. (before present) date by 1.030. All samples have been measured with extreme care, and almost every C^{14} date has been corrected for possible fractionation by the mass spectrometric measurement of C^{13}/C^{12} ratios.

In Figs. 43.1 to 43.6 and Fig. 43.8 we are presenting our new MASCA calibration curve. In order to eliminate some of the scatter in the raw data (see Fig. 43.7), this curve has been derived by 9-cell regression weighted averaging. Our choice of a 9-cell regression to smooth the raw radiocarbon data is based on much experimentation with other methods of smoothing (including a 5-cell regression and various fittings with polynomials). It soon became evident that the 9-cell floating average centered on its mid-point seemed to be the best choice since it resulted in a relatively smooth curve, but it did also preserve the major deviations and most of the minor ones expressed in the raw data of the three laboratories from which they were drawn.

Definitions

In the expianation of the tables that accompany the curve we shall deal with the terms "crossing," "span," and "range" as they pertain to the relationship of a specific radiocarbon date to the calibration curve. The majority of the corrections for radiocarbon dates (as read horizontally from the left-hand side of the graphs) are found to be single *crossings*. If the radiocarbon date follows the curve closely for a distance (usually a relatively short one) we designate this distance as a *span*. In those cases where a radiocarbon date crosses the curve two or more times, we must consider an overall *range*. This is essentially a range of uncertainty since the correction for the date may be derived from any point at which it *crosses* the curve. Of course, it is possible for a range to have a span and/or a crossing within the range.

Examples

An example of the use of the tables for a single crossing is as follows: The radiocarbon date of A.D. 1750 crosses the curve at the dendrochronologically determined date of A.D. 1650. Thus the correction for this date is 100 ± 10 years, the difference between A.D. 1750 and A.D. 1650. The ± 10 expresses the statistical uncertainty of the 9-cell regression average of the C^{14} dates and must be added to the standard statistical error (one sigma) of the radiocarbon date.

In Figs. 43.1 to 43.6 and 43.8, this uncertainty is represented by the diameter of the circles—namely, 20 years. In deriving the tables from the graphs, however, the corrected dates have been measured from mid-point to mid-point of the circles (on the horizontal scale). This is because the $\pm$ 10 uncertainty applies to the C^{14} dates (vertical axes) but not to the dendro-dates.

An example of our definition of range is the radiocarbon date A.D. 1680, which crosses the curve at the dendro-dates of A.D. 1610 and A.D. 1530. The correction in this case is either 70 years (the difference between A.D. 1680 and A.D. 1610) or 150 years (the difference between A.D. 1680 and A.D. 1530). In other words the range of uncertainty is 80 years, but one may not arbitrarily select the mid-point for the purpose of correction. Therefore, in the tables, we have not listed mid-points for these ranges.

An example of a span is the C^{14} date A.D. 1640. The span occurs from A.D. 1600 to A.D. 1510 and thus the true date could fall anywhere within this span.

The C^{14} date for A.D. 1790 has a range which includes both a span and a crossing. The span runs from A.D. 1760 to A.D. 1730 and then there is a crossing at A.D. 1660.

To take account of the uncertainty in a C^{14} date, one should first add and subtract the standard deviation plus the $\pm$ 10, and then find the maximum possible spread from the tables. For example, if one has a C^{14} date of 2000 ± 50 B.C., this becomes 2000 ± 60 B.C., or from 1940 to 2060 B.C. From Table IV one finds that the corrected spread is from 2190 to 2480 B.C.

The 631 radiocarbon dates of precisely dated woods included in this study spread over 661 C^{14} decades (A.D. 1849 to 4760 B.C.). These radiocarbon dates were determined from dendrochronologically dated samples which covered the total of 723 decades (A.D. 1849 to 5383 B.C.). In 560 (85 percent) of the C^{14} decades the correction is determined within a span of 20 to 50 years because the radiocarbon date crosses the curve only once or spans it for a distance of 50 years or less. These decades present a problem of lesser dimensions than the remaining 101 (15 percent) of the

decades in which the radiocarbon date crosses the curve more than once, or has spans, with the resultant range exceeding 50 years.

Application

In applying the results of this study for the correction or "calibration" of a radiocarbon date, the archaeologist or other user should be aware of both the strengths and weaknesses of the system as presented here. The statistics cited above point out that no difficulty will be encountered in correcting dates which fall into that 85 percent of the chronological period which does not contain deviations or where they are small. In cases of single crossings or short ranges or short spans, it must be remembered that the ± 10 years uncertainty of the single crossing is to be added to the standard statistical error of the radiocarbon date. Some of the more extensive ones will (or may) present special problems. Fortunately the two large deviations at the very beginning of the curve (Figs. 43.1 and 43.8) are not likely to be of much practical significance since the 17th to 19th centuries are well documented by sources other than radiocarbon dates. The large deviations that occur in the B.C. era, where other documentation is either scarce or non-existent present a greater difficulty. It will in reality be seldom that the archaeologist or historian will be able to find a reference point independent of the C^{14} dates which will pinpoint or narrow one's choice of a correction factor within the wider range of a large deviation. The Iron Age of Europe or the Late Bronze Age in many parts of the world may occasionally present separate confirmations for a choice. It is rare, even in the Late Neolithic of Europe, that one can differentiate phases of the period in terms of even, say, 200 years. Thus the amplitude of a deviation and therefore of a range of 90 years as in the period 4450 B.C. becomes less significant even when the standard statistical tolerance (one sigma) is added. With ± 60 attached to the date, this means that it falls somewhere between 5100 B.C. and 5240 B.C., a period of 140 years. And, that is all the accuracy that the bristlecone-based radiocarbon dates can provide for this particular period of time.

Geophysical Implications

As one can see in the figures presented here, there is no doubt that the long-term trend of deviations with a period of approximately 9000 years and a maximum amplitude (centered at about 4500 B.C.) of 10 percent is real. Curves have been derived by means of various computer programs to fit this trend, such as those of Damon (1972) and Suess (1973). Our best fit, based on 600 radiocarbon dates is shown as a dashed line in Figs. 43.1 to 43.6 and 43.8. It is derived from the following third order polynomial:

$$T_{C-14} = -43.96 + 0.918 \times T_D + 7.17 \times 10^{-5} \times T_D^2 + 1.18 \times 10^{-8} T_D^3$$

where T_{C-14} and T_D are positive for A.D. and negative for B.C.

$T_{C-14} = C^{14}$ date. T_D = dendro-date.

However, as we can see in the simple regression curve of Figs. 43.1 to 6 and 8, there are short-term deviations which depart from the main trend. It is our belief that some of the short-term deviations are real and that there is a tendency for cycles of 400 years but that there is no truly sinusoidal periodicity of such short periods. It might be noted, however, that the long-term deviation from a straight line could be close to a sinusoidal curve.

In regard to the basic causes of the deviations in atmospheric C^{14} contents, we think that part, but only part (Ralph 1972), of the long-term deviation is due to sinusoidal changes in the Earth's dipole moment which, in turn, may follow the recent pole reversal at about 13,000 B.P. (Barbetti and McElhinny 1972, Blakely and Cox 1972; Bonhommet and Zähringer 1969; Mörner, Lanser, and Hospers 1971); the short-term deviations may be related to variations in the Earth's nondipole field; there may be a contribution from the varying magnetic field of the Sun and the resultant interplanetary fields (Sawyer 1968) with cycles greater than eleven years; and as a secondary effect, from climatic changes (Houtermans, Suess, and Oeschger 1973); and there is a slight possibility of a contribution from the explosions of supernovae (Dergachev 1972).

ACKNOWLEDGEMENTS

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Richard D. Haynes of MASCA carefully checked Tables 1 to 6 and is also responsible for general editing of the *MASCA Newsletter.*

Barbara Lawn, assisted by Raymond L. Costa, Jr. and John D. Hedrick, of the Radiocarbon Laboratory processed the bristlecone samples that have been received by the laboratory over the past six years.

Douglas P. O'Brien of GeoMetrics, Palo Alto, California, derived the formula for the third polynomial used in Figs. 43.1 to 43.6 and Fig. 43.8.

David Wood of the Radiocarbon Laboratory worked out the smoothing methods and drafted the preliminary graphs indicated by the various methods.

We also acknowledge with gratitude financial support from the National Science Foundation for our known-age dating program. This has been supported since 1957 with NSF grants G-3281, G-5608, G-14094, GP-405, GP-3778, GA-993 and GA-12,572.

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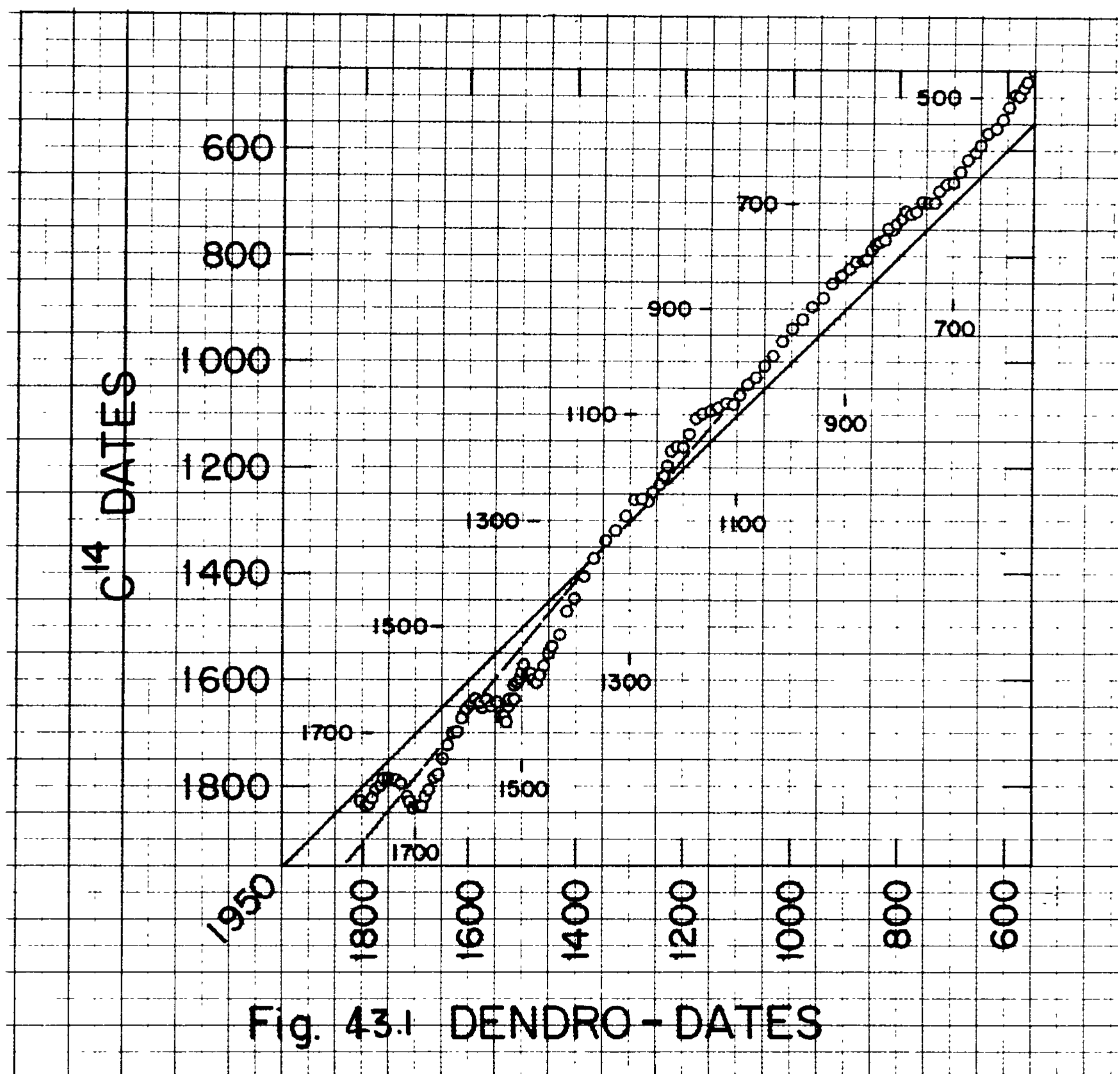


Fig. 43.1 DENDRO-DATES

SUPPLEMENT TO TABLE 1

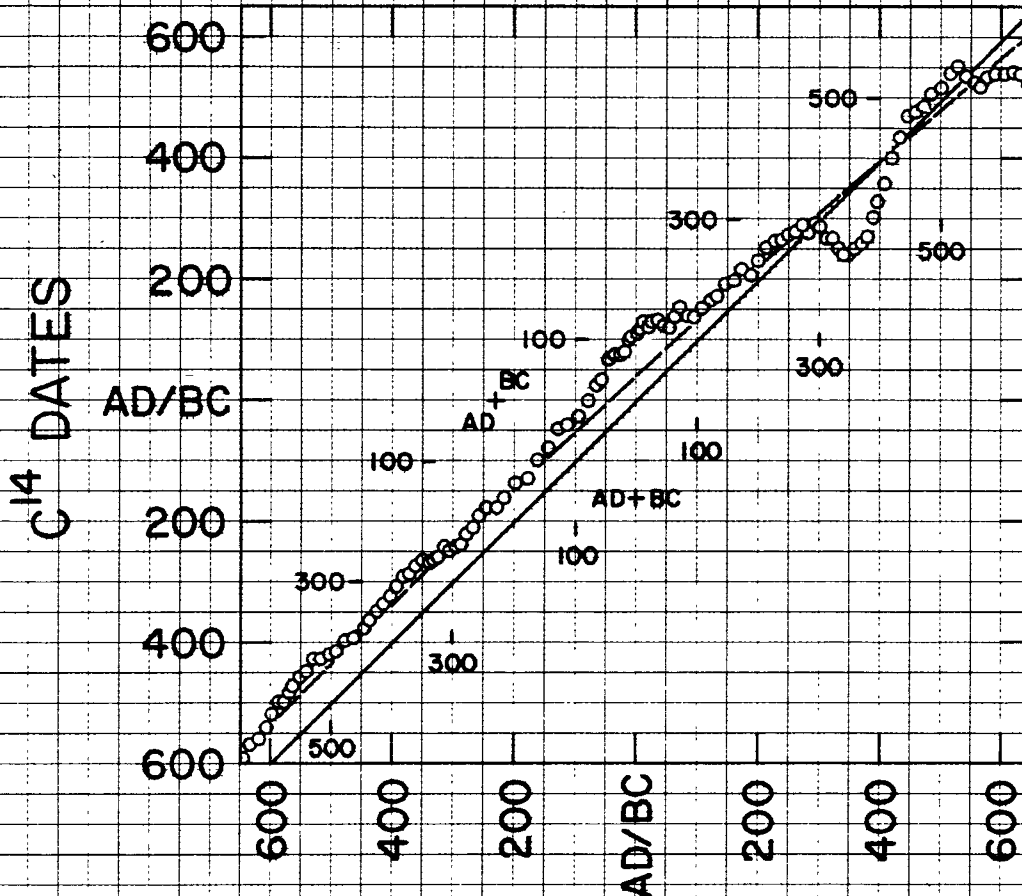
C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
AD 1840	AD 1790, 1710-1690	AD 1660	AD 1610, 1580-1520
AD 1830	AD 1800-1780, 1710-1680	AD 1650	AD 1610-1520
AD 1820	AD 1800-1780, 1710, 1680	AD 1640	AD 1600-1510
AD 1810	AD 1780, 1720, 1670	AD 1630	AD 1590-1510
AD 1800	AD 1770, 1730, 1670	AD 1610	AD 1520, 1470
AD 1790	AD 1760-1730, 1670	AD 1600	AD 1510-1470
AD 1780	AD 1760-1730, 1660	AD 1590	AD 1500-1470
AD 1690	AD 1630, 1530	AD 1580	AD 1500-1460
AD 1680	AD 1610, 1530	AD 1570	AD 1500, 1460
AD 1670	AD 1610, 1530	AD 1270	AD 1290-1260

(Continued on page 355)

*Simple crossings are indicated with entries separated by commas, spans are indicated with entries connected with hyphens.

TABLE 1

C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date
AD 1840	AD 1800-1690	AD 1540	AD 1440	AD 1240	AD 1260	AD 940	AD 1000
AD 1830	AD 1800-1680	AD 1530	AD 1440	AD 1230	AD 1250	AD 930	AD 1000-980
AD 1820	AD 1800-1680	AD 1520	AD 1430	AD 1220	AD 1240	AD 920	AD 980
AD 1810	AD 1780-1670	AD 1510	AD 1430	AD 1210	AD 1240	AD 910	AD 970
AD 1800	AD 1770-1670	AD 1500	AD 1430	AD 1200	AD 1230	AD 900	AD 960
AD 1790	AD 1760-1670	AD 1490	AD 1420	AD 1190	AD 1230	AD 890	AD 950
AD 1780	AD 1760-1660	AD 1480	AD 1420	AD 1180	AD 1220	AD 880	AD 940
AD 1770	AD 1660	AD 1470	AD 1420	AD 1170	AD 1220-1200	AD 870	AD 940
AD 1760	AD 1650	AD 1460	AD 1410	AD 1160	AD 1220-1200	AD 860	AD 930
AD 1750	AD 1650	AD 1450	AD 1410	AD 1150	AD 1210	AD 850	AD 920
AD 1740	AD 1650	AD 1440	AD 1410	AD 1140	AD 1190	AD 840	AD 910
AD 1730	AD 1640	AD 1430	AD 1400	AD 1130	AD 1190	AD 830	AD 910-890
AD 1720	AD 1640	AD 1420	AD 1390	AD 1120	AD 1180	AD 820	AD 890
AD 1710	AD 1630	AD 1410	AD 1390	AD 1110	AD 1180	AD 810	AD 880-860
AD 1700	AD 1630	AD 1400	AD 1390	AD 1100	AD 1180-1150	AD 800	AD 880-860
AD 1690	AD 1630-1530	AD 1390	AD 1380	AD 1090	AD 1170-1110	AD 790	AD 850
AD 1680	AD 1610-1530	AD 1380	AD 1380	AD 1080	AD 1140-1110	AD 780	AD 850-830
AD 1670	AD 1610-1530	AD 1370	AD 1370	AD 1070	AD 1120-1090	AD 770	AD 850-830
AD 1660	AD 1610-1520	AD 1360	AD 1360	AD 1060	AD 1100	AD 760	AD 820
AD 1650	AD 1610-1520	AD 1350	AD 1350	AD 1050	AD 1090	AD 750	AD 820-800
AD 1640	AD 1600-1510	AD 1340	AD 1350	AD 1040	AD 1080	AD 740	AD 820-800
AD 1630	AD 1590-1510	AD 1330	AD 1340	AD 1030	AD 1070	AD 730	AD 800-780
AD 1620	AD 1520	AD 1320	AD 1330	AD 1020	AD 1060	AD 720	AD 790-770
AD 1610	AD 1520-1470	AD 1310	AD 1320	AD 1010	AD 1050	AD 710	AD 790-770
AD 1600	AD 1510-1470	AD 1300	AD 1310	AD 1000	AD 1040	AD 700	AD 760-730
AD 1590	AD 1500-1470	AD 1290	AD 1310	AD 990	AD 1030	AD 690	AD 760-730
AD 1580	AD 1500-1460	AD 1280	AD 1300	AD 980	AD 1030	AD 680	AD 730
AD 1570	AD 1500-1460	AD 1270	AD 1290-1260	AD 970	AD 1020	AD 670	AD 730-700
AD 1560	AD 1450	AD 1260	AD 1290-1260	AD 960	AD 1020	AD 660	AD 720-700
AD 1550	AD 1450	AD 1250	AD 1290-1260	AD 950	AD 1010	AD 650	AD 690



SUPPLEMENT TO TABLE 2

C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
AD 610	AD 670-650	AD 290	AD 390-370
AD 570	AD 640-620	AD 280	AD 380-360
AD 560	AD 640-620	AD 270	AD 360-320
AD 500	AD 590-570	AD 260	AD 350-320
AD 490	AD 590-570	AD 250	AD 320-290
AD 430	AD 530-510	AD 240	AD 320-290
AD 420	AD 530-490	AD 180	AD 250-230
AD 410	AD 510-490	AD 170	AD 250-220
AD 400	AD 480-460	AD 130	AD 200-180
AD 390	AD 480-460	AD 50	AD 130-110

(Continued on page 355)

*Simple crossings are indicated with entries separated by commas, spans are indicated with entries connected with hyphens.

TABLE 2

C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date
AD 640	AD 690	AD 340	AD 410	AD 40	AD 130-110	260 BC	210-380 BC
AD 630	AD 680	AD 330	AD 400	AD 30	AD 110-90	270 BC	230-380 BC
AD 620	AD 670	AD 320	AD 400	AD 20	AD 100	280 BC	250-380 BC
AD 610	AD 670-650	AD 310	AD 390	AD 10	AD 90	290 BC	270-390 BC
AD 600	AD 660	AD 300	AD 390	AD1-1BC	AD 70	300 BC	270-390 BC
AD 590	AD 650	AD 290	AD 390-370	10 BC	AD 70	310 BC	390 BC
AD 580	AD 640	AD 280	AD 380-360	20 BC	AD 70	320 BC	400 BC
AD 570	AD 640-620	AD 270	AD 360-320	30 BC	AD 60	330 BC	400 BC
AD 560	AD 640-620	AD 260	AD 350-320	40 BC	AD 60	340 BC	400 BC
AD 550	AD 620	AD 250	AD 320-290	50 BC	AD 50	350 BC	410 BC
AD 540	AD 610	AD 240	AD 320-290	60 BC	AD 50	360 BC	410 BC
AD 530	AD 600	AD 230	AD 290	70 BC	AD 50-30	370 BC	410 BC
AD 520	AD 600	AD 220	AD 280	80 BC	AD 40-20	380 BC	410 BC
AD 510	AD 590	AD 210	AD 270	90 BC	AD 20	390 BC	420 BC
AD 500	AD 590-570	AD 200	AD 260	100 BC	AD 10	400 BC	420 BC
AD 490	AD 590-570	AD 190	AD 260	110 BC	AD 10-60 BC	410 BC	420 BC
AD 480	AD 570	AD 180	AD 250-230	120 BC	AD 10-60 BC	420 BC	430 BC
AD 470	AD 560	AD 170	AD 250-220	130 BC	10-100 BC	430 BC	430 BC
AD 460	AD 550	AD 160	AD 220	140 BC	10-100 BC	440 BC	430 BC
AD 450	AD 540	AD 150	AD 210	150 BC	70-110 BC	450 BC	440 BC
AD 440	AD 540	AD 140	AD 200	160 BC	70-120 BC	460 BC	440 BC
AD 430	AD 530-510	AD 130	AD 200-180	170 BC	120-140 BC	470 BC	440-460 BC
AD 420	AD 530-490	AD 120	AD 180	180 BC	130 BC	480 BC	440-470 BC
AD 410	AD 510-490	AD 110	AD 170	190 BC	140-160 BC	490 BC	470 BC
AD 400	AD 480-460	AD 100	AD 160	200 BC	140-190 BC	500 BC	480 BC
AD 390	AD 480-460	AD 90	AD 160	210 BC	170-190 BC	510 BC	490 BC
AD 380	AD 450	AD 80	AD 150	220 BC	170-200 BC	520 BC	500-640 BC
AD 370	AD 440	AD 70	AD 140	230 BC	200-250 BC	530 BC	500-640 BC
AD 360	AD 440	AD 60	AD 140	240 BC	210-360 BC	540 BC	510-660 BC
AD 350	AD 430	AD 50	AD 130-110	250 BC	210-370 BC	550 BC	510-660 BC

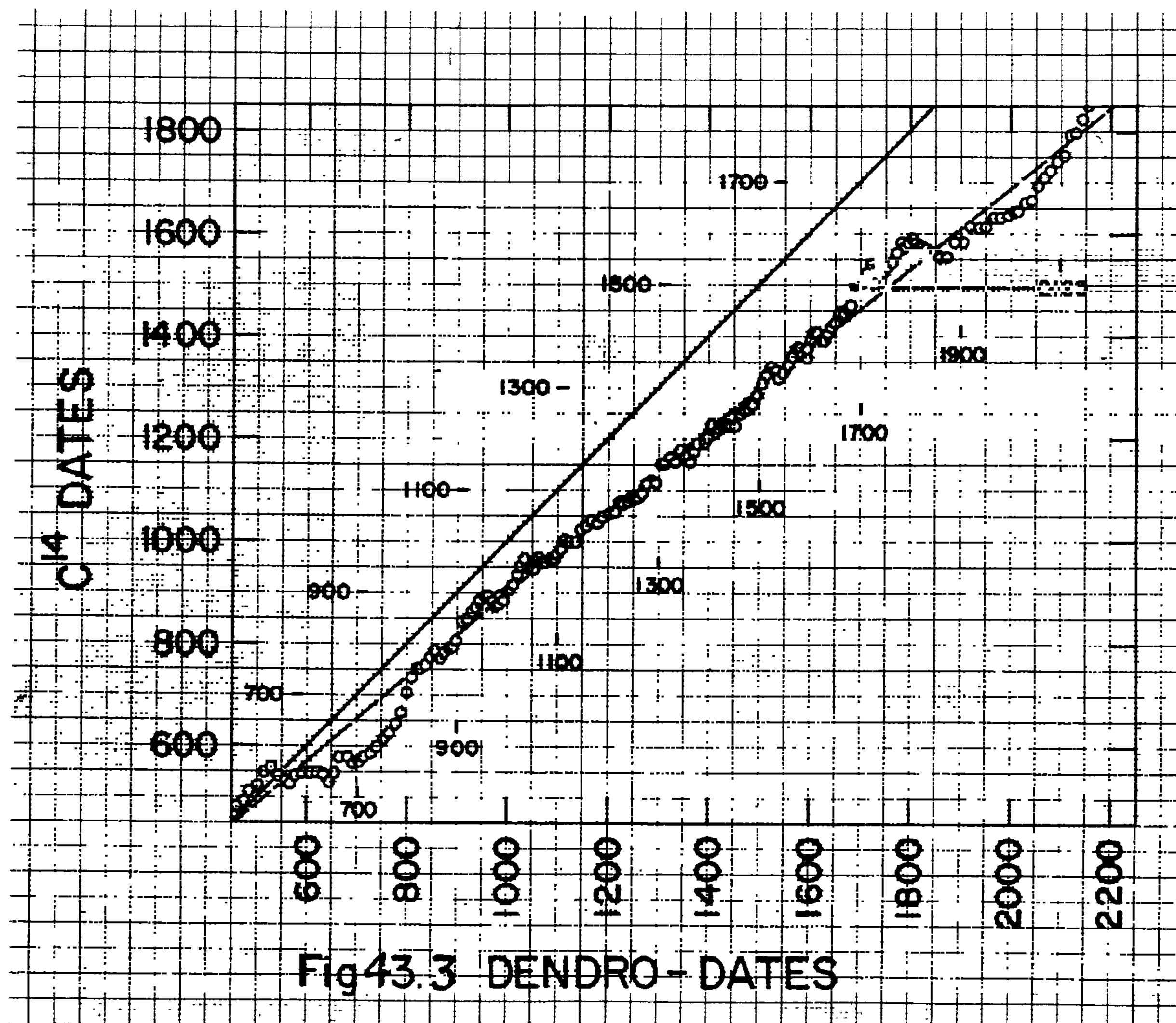


Fig43.3 DENDRO-DATES

SUPPLEMENT TO TABLE 3

C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
560 BC	530, 690-710 BC	850 BC	910-930 BC
570 BC	660-720 BC	870 BC	940, 980 BC
580 BC	660-730 BC	880 BC	940, 970-990 BC
590 BC	710-730 BC	890 BC	940-990 BC
750 BC	810-840 BC	900 BC	960, 980-1010 BC
760 BC	820-840 BC	940 BC	1020, 1C50 BC
770 BC	850-870 BC	950 BC	1030-1080 BC
780 BC	850-880 BC	960 BC	1030, 1060-1100 BC
790 BC	850-890 BC	970 BC	1030, 1060-1100 BC
800 BC	880-900 BC	1000 BC	1110-1140 BC

(Continued on page 356)

*Simple crossings are indicated with entries separated by commas, spans are indicated with entries connected with hyphens.

TABLE 3

C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date
560 BC	530-710 BC	860 BC	930 BC	1160 BC	1310-1360 BC	1460 BC	1680 BC
570 BC	660-720 BC	870 BC	940-980 BC	1170 BC	1320-1370 BC	1470 BC	1690 BC
580 BC	660-730 BC	880 BC	940-990 BC	1180 BC	1340-1370 BC	1480 BC	1690 BC
590 BC	710-730 BC	890 BC	940-990 BC	1190 BC	1370-1390 BC	1490 BC	1690-1710 BC
600 BC	740 BC	900 BC	960-1010 BC	1200 BC	1380-1400 BC	1500 BC	1690-1710 BC
610 BC	750 BC	910 BC	1010 BC	1210 BC	1400-1420 BC	1510 BC	1690-1730 BC
620 BC	760 BC	920 BC	1010 BC	1220 BC	1400-1450 BC	1520 BC	1710-1750 BC
630 BC	770 BC	930 BC	1020 BC	1230 BC	1400-1450 BC	1530 BC	1710-1750 BC
640 BC	780 BC	940 BC	1020-1050 BC	1240 BC	1460 BC	1540 BC	1720-1760 BC
650 BC	780 BC	950 BC	1030-1080 BC	1250 BC	1460-1480 BC	1550 BC	1720-1870 BC
660 BC	790 BC	960 BC	1030-1100 BC	1260 BC	1480 BC	1560 BC	1770-1870 BC
670 BC	790 BC	970 BC	1030-1100 BC	1270 BC	1490 BC	1570 BC	1770-1850 BC
680 BC	790 BC	980 BC	1100 BC	1280 BC	1500 BC	1580 BC	1780-1900 BC
690 BC	800 BC	990 BC	1110 BC	1290 BC	1500 BC	1590 BC	1780-1910 BC
700 BC	800 BC	1000 BC	1110-1140 BC	1300 BC	1510 BC	1600 BC	1800 BC
710 BC	800 BC	1010 BC	1110 BC	1310 BC	1500-1540 BC	1610 BC	1920-1950 BC
720 BC	800 BC	1020 BC	1150 BC	1320 BC	1510-1540 BC	1620 BC	1920-1950 BC
730 BC	810 BC	1030 BC	1160-1180 BC	1330 BC	1510-1550 BC	1630 BC	1960-2000 BC
740 BC	810 BC	1040 BC	1170-1190 BC	1340 BC	1520-1560 BC	1640 BC	1960-2020 BC
750 BC	820-840 BC	1050 BC	1170-1210 BC	1350 BC	1520-1590 BC	1650 BC	2000-2020 BC
760 BC	820-840 BC	1060 BC	1190-1210 BC	1360 BC	1560-1590 BC	1660 BC	2020-2040 BC
770 BC	850-870 BC	1070 BC	1220-1240 BC	1370 BC	1570-1600 BC	1670 BC	2040 BC
780 BC	850-880 BC	1080 BC	1220-1260 BC	1380 BC	1570-1600 BC	1680 BC	2050 BC
790 BC	850-890 BC	1090 BC	1240-1270 BC	1390 BC	1600-1630 BC	1690 BC	2050 BC
800 BC	880-900 BC	1100 BC	1270 BC	1400 BC	1600-1640 BC	1700 BC	2060 BC
810 BC	900 BC	1110 BC	1270-1300 BC	1410 BC	1600-1640 BC	1710 BC	2070 BC
820 BC	900 BC	1120 BC	1270-1300 BC	1420 BC	1640 BC	1720 BC	2080 BC
830 BC	900 BC	1130 BC	1290 BC	1430 BC	1650 BC	1730 BC	2080 BC
840 BC	910 BC	1140 BC	1300 BC	1440 BC	1660 BC	1740 BC	2090 BC
850 BC	910-930 BC	1150 BC	1300-1360 BC	1450 BC	1660-1680 BC	1750 BC	2110 BC

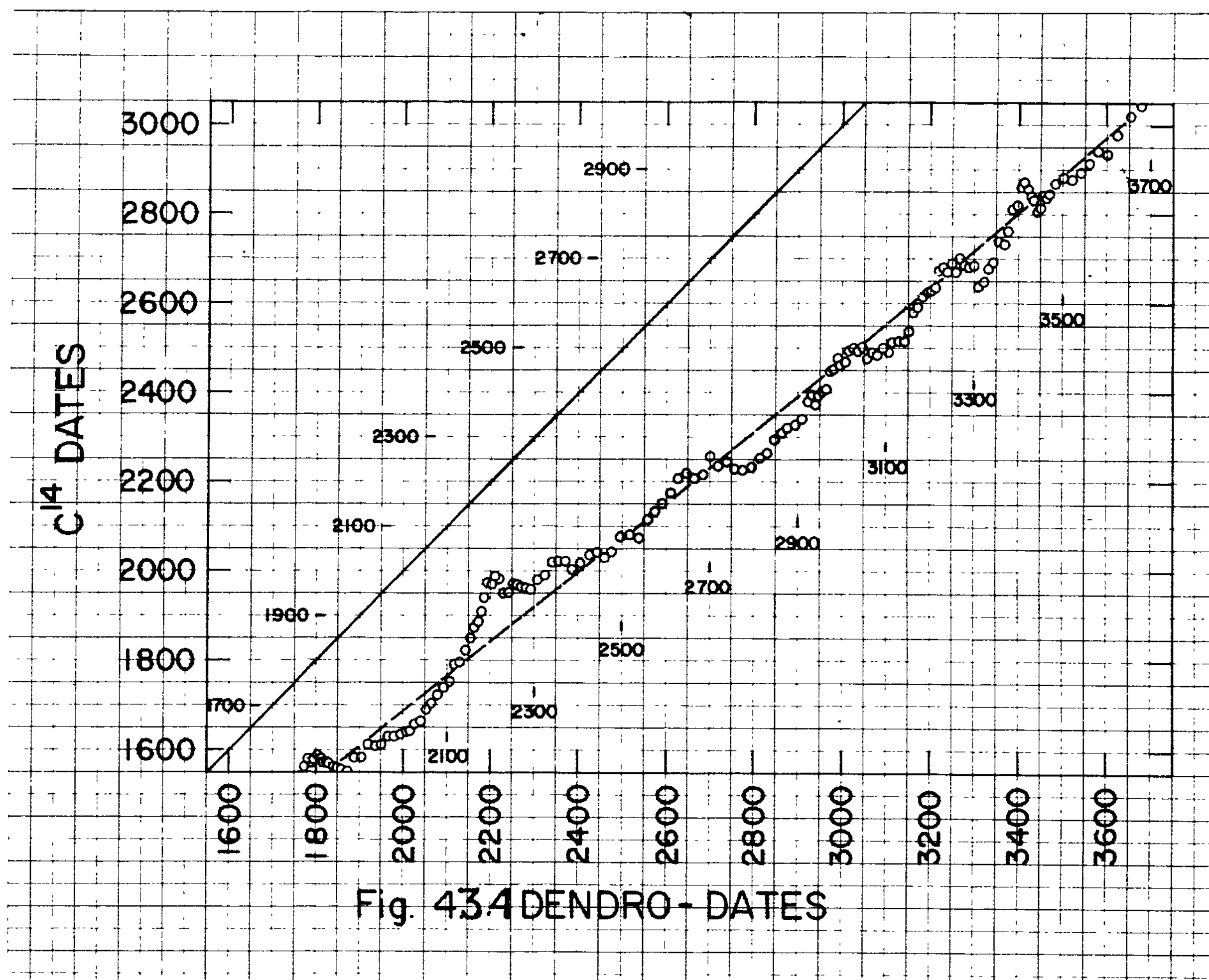


Fig. 434 DENDRO-DATES

SUPPLEMENT TO TABLE 4

C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
1790 BC	2110-2130 BC	2030 BC	2350-2370, 2430-2460 BC
1800 BC	2120-2140 BC	2040 BC	2420-2480 BC
1950 BC	2190, 2230, 2290 BC	2050 BC	2440, 2480 BC
1960 BC	2190, 2230-2290 BC	2070 BC	2490-2540 BC
1970 BC	2190-2210, 2250-2300 BC	2080 BC	2490-2540 BC
1980 BC	2190-2220, 2250, 2310 BC	2090 BC	2520, 2540 BC
1990 BC	2210, 2330 BC	2200 BC	2630, 2670 BC
2000 BC	2330, 2440 BC	2210 BC	2630-2680 BC
2010 BC	2340-2410 BC	2220 BC	2650, 2680, 2760-2780 BC
2020 BC	2340-2370, 2410, 2460 BC	2230 BC	2690, 2720, 2760-2800 BC

(Continued on page 356)

*Simple crossings are indicated with entries separated by commas, spans are indicated with entries connected with hyphens.

TABLE 4

C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date
1760 BC	2110 BC	2060 BC	2480 BC	2360 BC	2920 BC	2660 BC	3240-3330 BC
1770 BC	2110 BC	2070 BC	2490-2540 BC	2370 BC	2920-2940 BC	2670 BC	3220-3330 BC
1780 BC	2110 BC	2080 BC	2490-2540 BC	2380 BC	2920-2940 BC	2680 BC	3220-3330 BC
1790 BC	2110-2130 BC	2090 BC	2520-2540 BC	2390 BC	2930-2950 BC	2690 BC	3230-3340 BC
1800 BC	2120-2140 BC	2100 BC	2550 BC	2400 BC	2930-2960 BC	2700 BC	3250-3340 BC
1810 BC	2140 BC	2110 BC	2560 BC	2410 BC	2950-2970 BC	2710 BC	3270-3350 BC
1820 BC	2140 BC	2120 BC	2560 BC	2420 BC	2970 BC	2720 BC	3350 BC
1830 BC	2150 BC	2130 BC	2570 BC	2430 BC	2970 BC	2730 BC	3350-3370 BC
1840 BC	2150 BC	2140 BC	2580 BC	2440 BC	2970 BC	2740 BC	3350-3370 BC
1850 BC	2160 BC	2150 BC	2590 BC	2450 BC	2970-2990 BC	2750 BC	3370 BC
1860 BC	2160 BC	2160 BC	2600 BC	2460 BC	2980-3010 BC	2760 BC	3380 BC
1870 BC	2160 BC	2170 BC	2610 BC	2470 BC	2990-3060 BC	2770 BC	3380 BC
1880 BC	2170 BC	2180 BC	2610 BC	2480 BC	2990-3110 BC	2780 BC	3380 BC
1890 BC	2170 BC	2190 BC	2620 BC	2490 BC	3010-3110 BC	2790 BC	3380 BC
1900 BC	2180 BC	2200 BC	2630-2670 BC	2500 BC	3010-3110 BC	2800 BC	3390-3440 BC
1910 BC	2180 BC	2210 BC	2630-2680 BC	2510 BC	3030-3140 BC	2810 BC	3390-3450 BC
1920 BC	2180 BC	2220 BC	2650-2780 BC	2520 BC	3110-3140 BC	2820 BC	3390-3450 BC
1930 BC	2180 BC	2230 BC	2690-2800 BC	2530 BC	3150 BC	2830 BC	3400-3470 BC
1940 BC	2190 BC	2240 BC	2690-2800 BC	2540 BC	3150 BC	2840 BC	3400-3470 BC
1950 BC	2190-2290 BC	2250 BC	2700-2820 BC	2550 BC	3150 BC	2850 BC	3400-3470 BC
1960 BC	2190-2290 BC	2260 BC	2700-2830 BC	2560 BC	3160 BC	2860 BC	3400-3470 BC
1970 BC	2190-2300 BC	2270 BC	2830 BC	2570 BC	3160 BC	2870 BC	3410-3520 BC
1980 BC	2190-2310 BC	2280 BC	2840 BC	2580 BC	3160 BC	2880 BC	3410-3520 BC
1990 BC	2210-2330 BC	2290 BC	2850 BC	2590 BC	3170 BC	2890 BC	3500-3540 BC
2000 BC	2330-2440 BC	2300 BC	2850-2870 BC	2600 BC	3180 BC	2900 BC	3540 BC
2010 BC	2340-2410 BC	2310 BC	2860 BC	2610 BC	3180 BC	2910 BC	3560 BC
2020 BC	2340-2460 BC	2320 BC	2880-2900 BC	2620 BC	3180-3200 BC	2920 BC	3560 BC
2030 BC	2350-2460 BC	2330 BC	2880-2910 BC	2630 BC	3190-3310 BC	2930 BC	3570-3600 BC
2040 BC	2420-2480 BC	2340 BC	2910 BC	2640 BC	3210-3310 BC	2940 BC	3580-3600 BC
2050 BC	2440-2480 BC	2350 BC	2920 BC	2650 BC	3210-3320 BC	2950 BC	3580-3610 BC

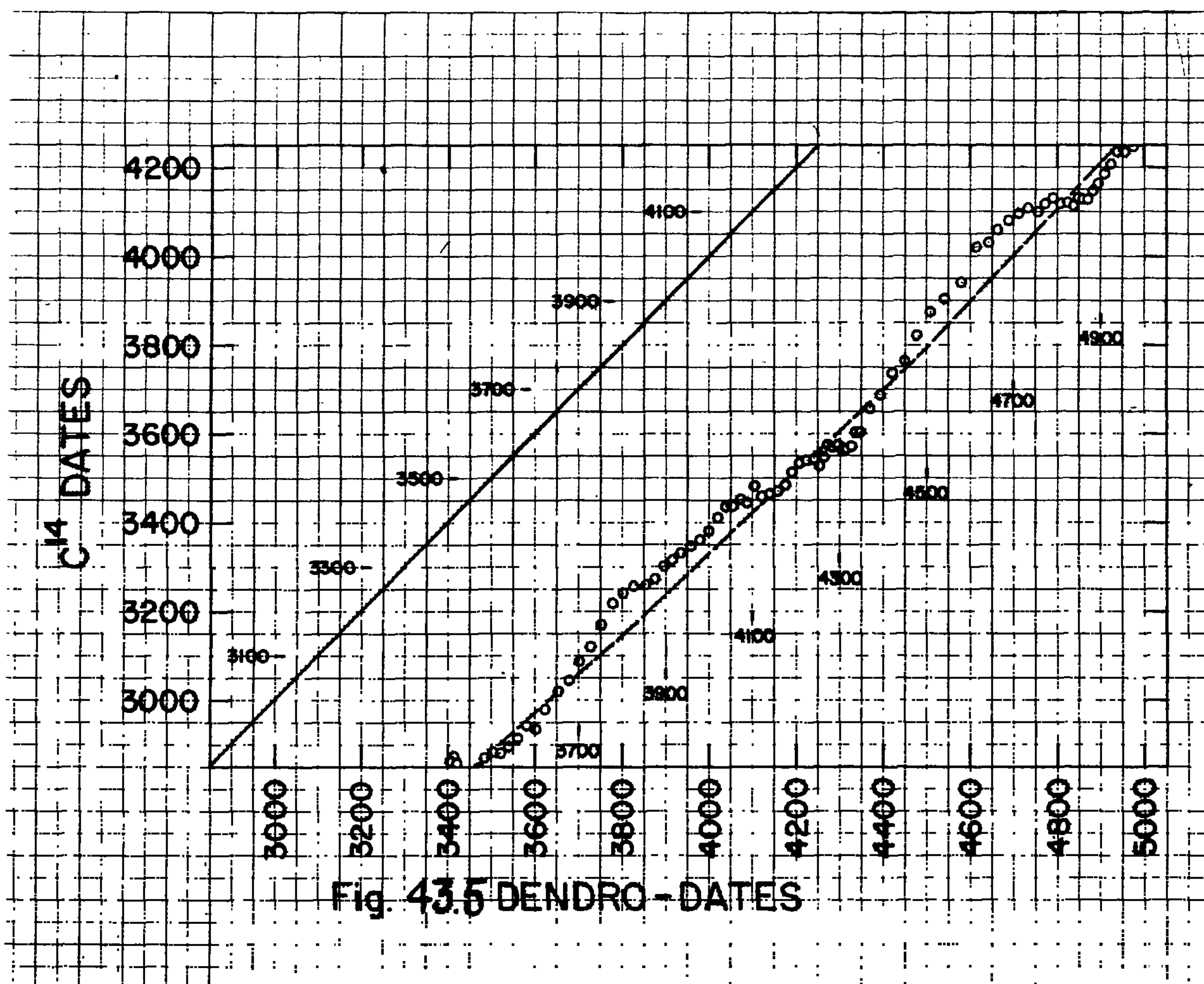


Fig. 43.5 DENDRO-DATES

SUPPLEMENT TO TABLE 5

C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
3250 BC	3800-3850 BC	3470 BC	4090, 4110, 4140-4160 BC
3260 BC	3820-3850 BC	3480 BC	4100, 4160-4180 BC
3270 BC	3850-3880 BC	3490 BC	4100, 4180 BC
3310 BC	3900-3920 BC	3520 BC	4190, 4250 BC
3340 BC	3940-3960 BC	3530 BC	4210-4250 BC
3350 BC	3960-3980 BC	3540 BC	4210-4260 BC
3430 BC	4040-4060 BC	3550 BC	4220-4270 BC
3440 BC	4040-4060, 4090 BC	3560 BC	4270, 4280-4330 BC
3450 BC	4070-4090, 4120 BC	3570 BC	4270-4330 BC
3460 BC	4070, 4090, 4120-4160 BC	3580 BC	4270-4300, 4330 BC

(Continued on page 355)

TABLE 5

C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date
2960 BC	3610 BC	3260 BC	3820-3850 BC	3560 BC	4270-4330 BC	3860 BC	4500 BC
2970 BC	3620 BC	3270 BC	3850-3880 BC	3570 BC	4270-4330 BC	3870 BC	4510 BC
2980 BC	3620 BC	3280 BC	3880 BC	3580 BC	4270-4330 BC	3880 BC	4520 BC
2990 BC	3630 BC	3290 BC	3890 BC	3590 BC	4330 BC	3890 BC	4530 BC
3000 BC	3640 BC	3300 BC	3900 BC	3600 BC	4330-4350 BC	3900 BC	4540 BC
3010 BC	3650 BC	3310 BC	3900-3920 BC	3610 BC	4330-4350 BC	3910 BC	4550 BC
3020 BC	3650 BC	3320 BC	3920 BC	3620 BC	4350 BC	3920 BC	4560 BC
3030 BC	3660 BC	3330 BC	3940 BC	3630 BC	4360 BC	3930 BC	4570 BC
3040 BC	3670 BC	3340 BC	3940-3960 BC	3640 BC	4360 BC	3940 BC	4580 BC
3050 BC	3680 BC	3350 BC	3960-3980 BC	3650 BC	4370 BC	3950 BC	4580 BC
3060 BC	3690 BC	3360 BC	3980 BC	3660 BC	4380 BC	3960 BC	4590 BC
3070 BC	3690 BC	3370 BC	3980 BC	3670 BC	4380 BC	3970 BC	4590 BC
3080 BC	3700 BC	3380 BC	4000 BC	3680 BC	4390 BC	3980 BC	4600 BC
3090 BC	3700 BC	3390 BC	4000 BC	3690 BC	4400 BC	3990 BC	4600 BC
3100 BC	3710 BC	3400 BC	4010 BC	3700 BC	4400 BC	4000 BC	4600 BC
3110 BC	3720 BC	3410 BC	4020 BC	3710 BC	4410 BC	4010 BC	4610 BC
3120 BC	3730 BC	3420 BC	4030 BC	3720 BC	4410 BC	4020 BC	4610-4640 BC
3130 BC	3730 BC	3430 BC	4040-4060 BC	3730 BC	4420 BC	4030 BC	4610-4640 BC
3140 BC	3740 BC	3440 BC	4040-4090 BC	3740 BC	4430 BC	4040 BC	4650 BC
3150 BC	3740 BC	3450 BC	4070-4120 BC	3750 BC	4430 BC	4050 BC	4660 BC
3160 BC	3750 BC	3460 BC	4070-4160 BC	3760 BC	4440 BC	4060 BC	4660 BC
3170 BC	3750 BC	3470 BC	4090-4160 BC	3770 BC	4450 BC	4070 BC	4670-4690 BC
3180 BC	3760 BC	3480 BC	4100-4180 BC	3780 BC	4460 BC	4080 BC	4690 BC
3190 BC	3760 BC	3490 BC	4100-4180 BC	3790 BC	4460 BC	4090 BC	4690-4760 BC
3200 BC	3770 BC	3500 BC	4190 BC	3800 BC	4470 BC	4100 BC	4710-4760 BC
3210 BC	3780 BC	3510 BC	4190 BC	3810 BC	4470 BC	4110 BC	4730-4840 BC
3220 BC	3780 BC	3520 BC	4190-4250 BC	3820 BC	4480 BC	4120 BC	4770-4870 BC
3230 BC	3790 BC	3530 BC	4210-4250 BC	3830 BC	4480 BC	4130 BC	4790-4870 BC
3240 BC	3800 BC	3540 BC	4210-4260 BC	3840 BC	4490 BC	4140 BC	4790-4880 BC
3250 BC	3800-3850 BC	3550 BC	4220-4270 BC	3850 BC	4490 BC	4150 BC	4880 BC

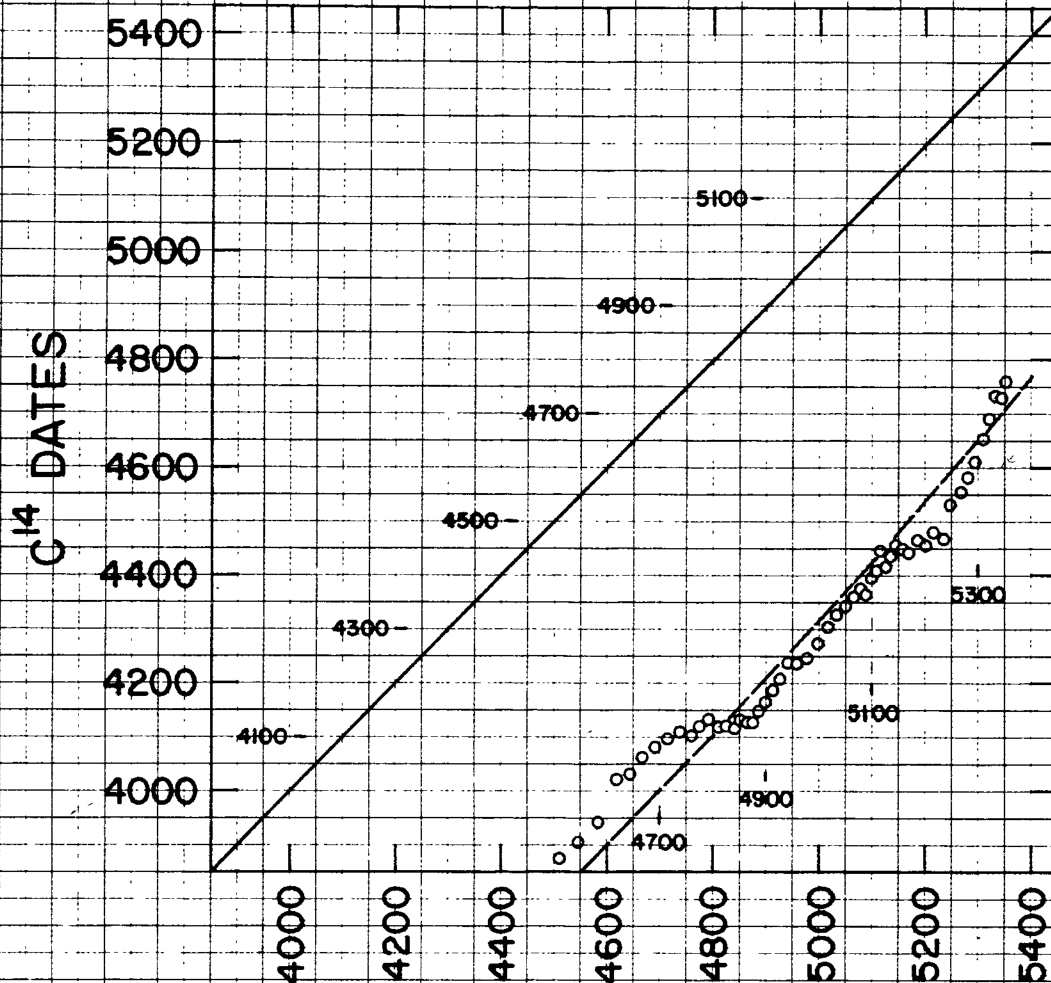


Fig.43.6

SUPPLEMENT TO TABLE 6

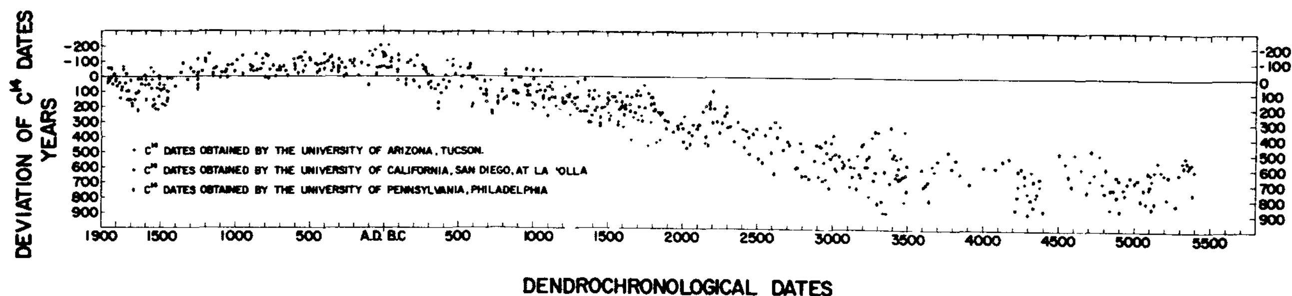
C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
4230 BC	4940-4960 BC	4450 BC	5110, 5140-5170, 5200 BC
4240 BC	4940-4980 BC	4460 BC	5140, 5180-5200, 5240 BC
4360 BC	5060-5090 BC	4470 BC	5180-5240 BC
4370 BC	5060-5090 BC	4480 BC	5210-5240 BC
4410 BC	5110-5130 BC	4490 BC	5210, 5240 BC
4420 BC	5110-5130 BC	4720 BC	5330-5350 BC
4430 BC	5110-5130 BC	4730 BC	5330-5350 BC
4440 BC	5110-5170 BC	4740 BC	5330-5350 BC

*Simple crossings are indicated with entries separated by commas, spans are indicated with entries connected with hyphens.

TABLE 6

C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date	C-14 Date	Range or Mid-Point for Corrected Date
4160 BC	4900 BC	4360 BC	5060-5090 BC	4560 BC	5270 BC	4760 BC	5350 BC
4170 BC	4900 BC	4370 BC	5060-5090 BC	4570 BC	5280 BC		
4180 BC	4910 BC	4380 BC	5080 BC	4580 BC	5280 BC		
4190 BC	4910 BC	4390 BC	5100 BC	4590 BC	5280 BC		
4200 BC	4920 BC	4400 BC	5100 BC	4600 BC	5290 BC		
4210 BC	4930 BC	4410 BC	5110-5130 BC	4610 BC	5290 BC		
4220 BC	4930 BC	4420 BC	5110-5130 BC	4620 BC	5300 BC		
4230 BC	4940-4960 BC	4430 BC	5110-5140 BC	4630 BC	5300 BC		
4240 BC	4940-4980 BC	4440 BD	5110-5170 BC	4640 BC	5310 BC		
4250 BC	4980 BC	4450 BC	5110-5200 BC	4650 BC	5310 BC		
4260 BC	4990 BC	4460 BC	5140-5240 BC	4660 BC	5310 BC		
4270 BC	5000 BC	4470 BC	5180-5240 BC	4670 BC	5320 BC		
4280 BC	5000 BC	4480 BC	5210-5240 BC	4680 BC	5320 BC		
4290 BC	5010 BC	4490 BC	5210-5240 BC	4690 BC	5320 BC		
4300 BC	5020 BC	4500 BC	5240 BC	4700 BC	5330 BC		
4310 BC	5020 BC	4510 BC	5240 BC	4710 BC	5330 BC		
4320 BC	5030 BC	4520 BC	5250 BC	4720 BC	5330-5350 BC		
4330 BC	5030 BC	4530 BC	5250 BC	4730 BC	5330-5350 BC		
4340 BC	5050 BC	4540 BC	5260 BC	4740 BC	5330-5350 BC		
4350	5050 BC	4550 BC	5270 BC	4750 BC	5350 BC		

FIGURE 43.7. *Individual C-14 dates for dendro-dated samples.*



SUPPLEMENT TO TABLE 1 (Cont.)

C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
AD 1260	AD 1290-1260	AD 780	AD 850-830
AD 1250	AD 1290-1260	AD 770	AD 850-830
AD 1170	AD 1220-1200	AD 750	AD 820-800
AD 1160	AD 1220-1200	AD 740	AD 820-800
AD 1100	AD 1180-1150	AD 730	AD 800-780
AD 1090	AD 1170-1110	AD 720	AD 790-770
AD 1080	AD 1140-1110	AD 710	AD 790-770
AD 1070	AD 1120-1090	AD 700	AD 760-730
AD 930	AD 1000-980	AD 690	AD 760-730
AD 830	AD 910-890	AD 670	AD 730-700
AD 810	AD 880-860	AD 660	AD 720-700
AD 800	AD 880-860		

SUPPLEMENT TO TABLE 2 (Cont.)

C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
AD 40	AD 130-110	230 BC	200, 350 BC
AD 30	AD 120-90		
70 BC	AD 50-30	240 BC	210, 340-360 BC
80 BC	AD 40-20	250 BC	210, 330-370 BC
110 BC	AD 10-20 BC, 60 BC	260 BC	210, 330, 380 BC
120 BC	AD 10-60 BC	270 BC	230-380, 310-330, 380 BC
130 BC	10-100 BC	280 BC	250-300, 380 BC
140 BC	10-40, 60-100 BC	290 BC	270-300, 390 BC
150 BC	70-110 BC	300 BC	270-290, 390 BC
160 BC	70, 120 BC	470 BC	440-460 BC
170 BC	120-140 BC	480 BC	440-470 BC
190 BC	140-160 BC	520 BC	500, 570, 640 BC
200 BC	140-160, 190 BC	530 BC	500, 570, 640 BC
210 BC	170-190 BC	540 BC	510, 540-660 BC
220 BC	170, 200 BC	550 BC	510-540, 570-660 BC

SUPPLEMENT TO TABLE 5 (Cont.)

C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
3600 BC	4330-4350 BC	4100 BC	4710-4760 BC
3610 BC	4330-4350 BC	4110 BC	4730-4780, 4800-4840 BC
4020 BC	4610-4640 BC	4120 BC	4770-4870 BC
4030 BC	4610-4640 BC	4130 BC	4790-4870 BC
4070 BC	4670-4690 BC	4140 BC	4790, 4850-4880 BC
4090 BC	4690-4710, 4760 BC		

*Simple crossings are indicated with entries separated by commas, spans are indicated with entries connected with hyphens

SUPPLEMENT TO TABLE 3 (Cont.)

C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
1030 BC	1160-1180 BC	1370 BC	1570-1600 BC
1040 BC	1170-1190 BC	1380 BC	1570-1600 BC
1050 BC	1170-1210 BC	1390 BC	1600-1630 BC
1060 BC	1190-1210 BC	1400 BC	1600-1640 BC
1070 BC	1220-1240 BC	1410 BC	1600-1640 BC
1080 BC	1220-1260 BC	1450 BC	1660-1680 BC
1090 BC	1240-1270 BC	1490 BC	1690-1710 BC
1110 BC	1270-1300 BC	1500 BC	1690-1710 BC
1120 BC	1270-1300 BC	1510 BC	1690, 1730 BC
1150 BC	1300-1330, 1360 BC	1520 BC	1710-1750 BC
1160 BC	1310-1360 BC	1530 BC	1710-1750 BC
1170 BC	1320-1370 BC	1540 BC	1720-1760 BC
1180 BC	1340, 1370 BC	1550 BC	1720, 1760, 1870 BC
1190 BC	1370-1390 BC	1560 BC	1770, 1840-1870 BC
1200 BC	1380-1400 BC	1570 BC	1770, 1830-1850 BC
1210 BC	1400-1420 BC	1580 BC	1780-1820, 1880-1900 BC
1220 BC	1400-1450 BC	1590 BC	1780-1800 BC, 1890-1910 BC
1230 BC	1400-1450 BC	1610 BC	1920-1950 BC
1250 BC	1460-1480 BC	1620 BC	1920-1950 BC
1310 BC	1500, 1540 BC	1630 BC	1960-2000 BC
1320 BC	1510, 1540 BC	1640 BC	1960-2020 BC
1330 BC	1510-1550 BC	1650 BC	2000-2020 BC
1340 BC	1520-1560 BC	1660 BC	2020-2040 BC
1350 BC	1520, 1560-1590 BC	1750 BC	2090-2120 BC
1360 BC	1560-1590 BC		

SUPPLEMENT TO TABLE 4 (Cont.)

C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*	C-14 Date	Points in "Range for Corrected Date" in which C-14 date crosses curve or follows it for a distance*
2240 BC	2690, 2710-40, 2800 BC	2650 BC	3210, 3320 BC
2250 BC	2700, 2740, 2820 BC	2660 BC	3240-3260, 3300, 3330 BC
2260 BC	2700, 2810-2830 BC	2670 BC	3220-3260, 3290, 3300, 3330 BC
2300 BC	2850-2870 BC	2680 BC	3220-3250, 3280-3300, 3330 BC
2320 BC	2880-2900 BC	2690 BC	3230-3300, 3340 BC
2330 BC	2880-2910 BC	2700 BC	3250-3270, 3340 BC
2370 BC	2920-2940 BC	2710 BC	3270, 3350 BC
2380 BC	2920-2940 BC	2730 BC	3350-3370 BC
2390 BC	2930-2950 BC	2740 BC	3350-3370 BC
2400 BC	2930-2960 BC	2800 BC	3390, 3440 BC
2410 BC	2950-2970 BC	2810 BC	3390, 3450 BC
2450 BC	2970-2990 BC	2820 BC	3390, 3450 BC
2460 BC	2980-3010 BC	2830 BC	3400, 3430, 3470 BC
2470 BC	2990-3010, 3060 BC	2840 BC	3400, 3430-3470 BC
2480 BC	2990, 3050-3080, 3110 BC	2850 BC	3400, 3430, 3450-3470 BC
2490 BC	3010-3040, 3070-3110 BC	2860 BC	3400-3420, 3470 BC
2500 BC	3010-3050, 3090-3110 BC	2870 BC	3410, 3480, 3520 BC
2510 BC	3030-3050, 3090-3140 BC	2880 BC	3410, 3500-3520 BC
2520 BC	3110-3140 BC	2890 BC	3500, 3540 BC
2620 BC	3180-3200 BC	2930 BC	3570, 3600 BC
2630 BC	3190-3210, 3310 BC	2940 BC	3580-3600 BC
2640 BC	3210, 3310 BC	2950 BC	3580, 3610 BC

*Simple crossings are indicated with entries separated by commas, spans are indicated with entries connected with hyphens.

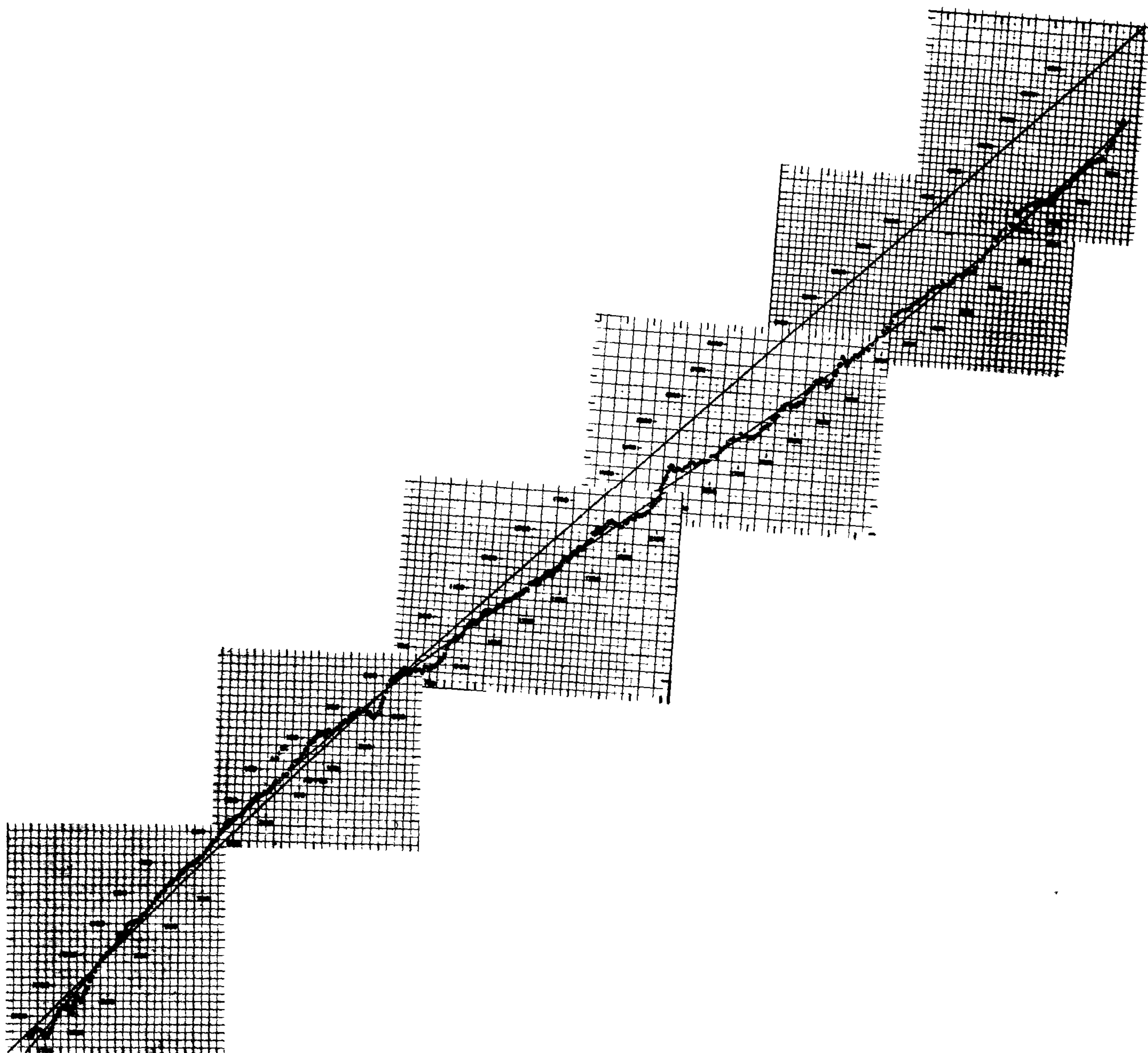


FIGURE 43.8. *Composit plot of Figures 43.1 through 6.*

Radiocarbon Dates for the Indus Civilization and Related Sites

Site and Context		Laboratory Number	Date 5568 ± 30 Years Ago	Date 5730 ± 40 Years Ago	Years B.C. (5730 ± 40)	MASCA Correction after Ralph, Michael and Han 1973
ALLAHDINO						
level I (– 60 cm)		P-2237	3840 ± 60	3960 ± 63	2010 ± 63	2540-2190 B.C.
level I (– 40 cm)		P-2295	3760 ± 70	3880 ± 73	1930 ± 73	2410-2160 B.C.
level I (– 60 cm)		P-2296	3930 ± 50	4050 ± 53	2100 ± 53	2600-2420 B.C.
AMRI						
Mound A, layer 19		TF 863	4585 ± 108	4723 ± 111	2773 ± 111	3540-3210 B.C.
Mound A, layer 28c		TF 864	4709 ± 108	4850 ± 111	2900 ± 111	3650-3380 B.C.
DAMB SADAAT						
Period	I	UW-59	4330 ± 70	4460 ± 72	2510 ± 72	3180-2970 B.C.
	I	L-180b	4338 ± 350	4468 ± 361	2518 ± 361	3540-2590 B.C.
	II	P-522	4378 ± 196	4509 ± 202	2559 ± 202	3380-2920 B.C.
	II	P-523	4029 ± 74	4150 ± 76	2200 ± 76	2850-2560 B.C.
	II	L-180c	4375 ± 412	4506 ± 425	2556 ± 425	3630-2560 B.C.
	II	L-180e	4367 ± 361	4498 ± 372	2548 ± 372	3600-2610 B.C.
	III	UW-60	4030 ± 160	4151 ± 165	2201 ± 165	2940-2350 B.C.
GUMLA						
early level of Period II		P-1812	4076 ± 72	4198 ± 74	2248 ± 74	2910-2600 B.C.
HATALA layer 2		P-1813	4043 ± 60	4164 ± 62	2214 ± 62	2850-2580 B.C.
KALIBANGAN						
Period	II	P-481	3879 ± 72	3995 ± 74	2045 ± 74	2570-2190 B.C.
	II	TF 25	3930 ± 110	4048 ± 113	2098 ± 113	2780-2190 B.C.
	II	TF 138	3075 ± 100	3167 ± 103	1217 ± 103	1550-1270 B.C.
	II	TF 139	3775 ± 100	3888 ± 103	1938 ± 103	2480-2140 B.C.
	II	TF 141	3705 ± 110	3816 ± 113	1866 ± 113	2330-2090 B.C.
	II	TF 142	3635 ± 100	3744 ± 103	1794 ± 103	2180-2050 B.C.
	II	TF 143	3510 ± 110	3615 ± 113	1665 ± 113	2130-1720 B.C.
	II	TF 145	3895 ± 100	4012 ± 103	2062 ± 103	2610-2190 B.C.
	II	TF 147	2865 ± 100	2951 ± 103	1001 ± 103	1300-940 B.C.
	II	TF 149	3675 ± 140	3785 ± 144	1835 ± 144	2330-2050 B.C.
	II	TF 150	3740 ± 100	3852 ± 103	1902 ± 103	2460-2110 B.C.
	II	TF 151	3800 ± 100	3914 ± 103	1964 ± 103	2540-2160 B.C.

Site and Context		Laboratory Number	Date 5568 ± 30 Years Ago	Date 5730 ± 40 Years Ago	Years B.C. (5730 ± 40)	MASCA Correction after Ralph, Michael and Han 1973
KALIBANGAN *Contd.*						
Period	II	TF 152	3615 ± 85	3723 ± 87	1773 ± 87	2160-2050 B.C.
	II	TF 153	3910 ± 110	4027 ± 113	2077 ± 113	2670-2190 B.C.
	I	TF 154	3665 ± 110	3775 ± 113	1825 ± 113	2290-2060 B.C.
	I	TF 155	4195 ± 115	4321 ± 118	2371 ± 118	3110-2690 B.C.
	I	TF 156	3740 ± 105	3852 ± 108	1902 ± 108	2460-2110 B.C.
	I	TF 157	4120 ± 100	4244 ± 103	2294 ± 103	2970-2610 B.C.
	II	TF 160	4060 ± 100	4182 ± 103	2232 ± 103	2920-2560 B.C.
	I	TF 161	3930 ± 100	4048 ± 103	2098 ± 103	2680-2210 B.C.
	I	TF 162	3940 ± 100	4058 ± 103	2108 ± 103	2780-2330 B.C.
	II	TF 163	3910 ± 100	4027 ± 103	2077 ± 103	2620-2190 B.C.
	I	TF 165	3800 ± 100	3914 ± 103	1964 ± 103	2540-2160 B.C.
	I	TF 240	3610 ± 100	3718 ± 103	1768 ± 103	2170-2000 B.C.
	I	TF 241	4090 ± 90	4213 ± 93	2263 ± 93	2940-2600 B.C.
	II	TF 244	3250 ± 90	3347 ± 93	1397 ± 93	1710-1500 B.C.
	II	TF 942	4055 ± 108	4177 ± 111	2227 ± 111	2920-2560 B.C.
	II	TF 607	3930 ± 120	4048 ± 124	2094 ± 124	2800-2190 B.C.
	II	TF 608	3910 ± 108	4027 ± 111	2077 ± 111	2670-2190 B.C.
	II	TF 948	3816 ± 100	3930 ± 103	1980 ± 103	2540-2160 B.C.
	II	TF 605	3783 ± 97	3896 ± 100	1946 ± 100	2480-2150 B.C.
	II	TF 947	3757 ± 85	3870 ± 88	1920 ± 88	2460-2140 B.C.
	II	TF 946	3605 ± 100	3713 ± 103	1763 ± 103	2170-2000 B.C.
KILI GUL MOHAMMED						
Period	I	P-524	5474 ± 83	5638 ± 85	3688 ± 85	4460-4330 B.C.
	I	UW-61	5260 ± 80	5418 ± 82	3468 ± 82	4330-4000 B.C.
	I	L-180a	5497 ± 500	5662 ± 515	3712 ± 515	4980-3760 B.C.
KOT DIJI						
Citadel, level 14		P-196	4421 ± 141	4554 ± 145	2604 ± 145	3380-2970 B.C.
Citadel, level 4		P-195	3925 ± 134	4043 ± 138	2093 ± 138	2800-2190 B.C.
Lower City, level 5		P-180	4083 ± 137	4205 ± 141	2255 ± 141	2970-2550 B.C.
Lower City, level 5		P-179	4161 ± 151	4286 ± 156	2336 ± 156	3110-2610 B.C.
LOTHAL						
Period	B	TF 19	3650 ± 135	3759 ± 139	1809 ± 139	2290-2020 B.C.
	A	TF 22	3845 ± 110	3960 ± 113	2010 ± 113	2570-2170 B.C.
	B	TF 23	3705 ± 105	3816 ± 108	1866 ± 108	2310-2110 B.C.
	A	TF 26	3830 ± 120	3945 ± 123	1995 ± 123	2570-2160 B.C.
	A	TF 27	3840 ± 110	3955 ± 113	2005 ± 113	2570-2170 B.C.
	A	TF 29	3740 ± 110	3852 ± 113	1902 ± 113	2460-2110 B.C.
	A	TF 113	3740 ∓ 110	3852 ∓ 113	1902 ∓ 113	2460-2110 B.C
	A	TF 135	3405 ± 125	3507 ± 128	1557 ± 128	2060-1640 B.C.
	A	TF 136	3915 ± 130	4032 ± 133	2082 ± 133	2800-2190 B.C.
	B	TF 110	3739 ± 108	3851 ± 111	1901 ± 111	2460-2110 B.C.
MOHENJO DARO						
Late levels		TF 75	3600 ± 110	3708 ± 113	1758 ± 113	2170-1960 B.C.
Mature levels		P-1176	3801 ± 59	3915 ± 61	1965 ± 61	2480-2170 B.C.
,,		P-1177	3895 ± 64	4012 ± 66	2062 ± 66	2580-2210 B.C.
,,		P-1178a	3802 ± 59	3916 ± 61	1966 ± 61	2480-2170 B.C.
,,		P-1179	3913 ± 64	4030 ± 66	2080 ± 66	2600-2330 B.C.
,,		P-1180	3828 ± 61	3943 ± 63	1993 ± 63	2540-2180 B.C.
,,		P-1182a	3702 ± 63	3813 ± 65	1863 ± 65	2190-2110 B.C.

Site and Context	Laboratory Number	Date 5568 ± 30 Years Ago	Date 5730 ± 40 Years Ago	Years B.C. (5730 ± 40)	MASCA Correction after Ralph, Michael and Han 1973
MUNDIGAK					
Period I, 2-3	TF 1129	4947 ± 107	5095 ± 110	3145 ± 110	3880-3650 B.C.
Period I, 5	TF 1131	4568 ± 102	4705 ± 105	2755 ± 105	3520-3210 B.C.
Period II, 1 or Period I, 5	TF 1132	2908 ± 102	4945 ± 105	2995 ± 105	3720-3410 B.C.
NIAI BUTHI	P-478	3740 ± 64	3850 ± 65	1700 ± 65	2110-1770 B.C.
ROJDI					
Period I	TF 199	3590 ± 100	3698 ± 103	1748 ± 103	2160-1960 B.C.
Period I	TF 200	3810 ± 110	3924 ± 113	1974 ± 113	2550-2160 B.C.
SOMNATH					
D 6 (13)	TF 1286	3597 ± 92	3705 ± 94	1755 ± 94	2160-2000 B.C.
D 6 (12)	TF 1285	3461 ∓ 97	3565 ± 99	1615 ± 99	2080-1690 B.C.
C 4 (10)	PRL-91	3854 ± 165	3970 ± 170	2020 ± 170	2670-2150 B.C.
D 6-7 (14)	PRL-92	3825 ± 97	3940 ± 100	1990 ± 100	2550-1870 B.C.
B 5 (15)	TF 1287	4267 ± 107	4395 ± 110	2445 ± 110	3160-2880 B.C.
D 6 (16) flood deposit	PRL-90	4233 ± 112	4360 ± 115	2410 ± 115	3150-2840 B.C.
SURKOTADA					
layer 3	TF-1294	3620 ± 95	3729 ± 98	1779 ± 98	2050-2170 B.C.
layer 8	TF-1295	3780 ± 95	3893 ± 98	1943 ± 98	2160-2480 B.C.
pit 1 sealed by layer 3	TF-1297	3635 ± 95	3744 ± 98	1794 ± 98	2060-2170 B.C.
layer 17	TF-1301	3840 ± 130	3955 ± 134	2005 ± 134	2160-2480 B.C.
layer 18a and 20	TF-1304/ 1309	3645 ± 90	3754 ± 93	1804 ± 93	2070-2180 B.C.
layer 19	TF-1305	3890 ± 95	4007 ± 98	2057 ± 98	2190-2600 B.C.
layer 5	TF-1307	3510 ± 105	3615 ± 108	1665 ± 108	1770-2110 B.C.
layer 11	TF-1310	3810 ± 95	3924 ± 98	1974 ± 98	2170-2540 B.C.

Part XI An Extensive Bibliography of the Indus Civilization including References Cited in the Text

Editor's Introduction

The final part of this book is a comprehensive bibliography of the Indus Civilization. In addition, all references cited by the various authors have been integrated into the bibliography whether they are or not of direct relevance to the civilization. Thus in its fullest form it is both a bibliography of the Indus Civilization and a source on ancient civilizations generally.

The bibliography has been organized alphabetically by author in standard fashion. This facilitates use by those familiar with the general literature on things Indus, as well as those who wish to check individual citations from the papers which have been reproduced. In addition there is a final section of the bibliography which has been topically organized. This will be of utility for those who wish to use this material as a research tool.

Book reviews and newspaper articles have not been included unless they contain something of general interest. The bibliography by B.M. Pande and K.S. Ramachandran (1971) should be consulted if these aspects of documentation are sought, since it is particularly strong in these areas. The decision to be selective here was made to retain control over the size of this section and yet cover literature of genuine interest to both the expert and general reader.

Two other bibliographies ought to be noted. The first by Robert Brunswig (1974) is comprehensive and topically organized. In that form it is of considerable use to those unfamiliar with this literature. The second by D. King (1975) is a bibliography of Pakistan archaeology which includes many references to the Indus Civilization.

Abbreviations

AASOR	Annual of the American Schools of Oriental Research, New Haven.
ABIA	Annual Bibliography of Indian Archaeology, Kern Institute, Leiden.
ABORI	Annals of the Bhandarkar Oriental Research Institute, Poona.
ADIM	American Documentation Institute, c/o Photoduplication Service, Library of Congress.
AI	Ancient India, Bulletin of the Archaeological Survey of India, New Delhi.
AIOC	All-India Oriental Conference.
AJ	The Antiquaries Journal, the Journal of the Society of Antiquaries, London.
AJA	American Journal of Archaeology, Cincinnati.
ALB	Adyar Library Bulletin, Madras.
Am An	American Anthropologist, Beloit.
AMN	American Museum Novitates, New York.
An Pa	Ancient Pakistan, Peshawar.
Anthropos	Anthropos, Revue Internationale d'Ethnologie et de Linguistique, Fribourg.
Antiquity	Antiquity, A Quarterly Review of Archaeology, Cambridge, England.
AO	Archiv Orientalni, Prague.
AP	Asian Perspectives, Bulletin of Far Eastern Prehistory Association, Hongkong/Honolulu.
APAMNH	Anthropological Papers of the American Museum of Natural History, New York.
APS	American Philosophical Society, Philadelphia.
AR	Asian Review, London.
ARASI	Annual Report of the Archaeological Survey of India, Delhi.
Archaeology	Archaeology, New York.
Art As.	Arts Asiatiques, Paris.
Artibus Asiae	Artibus Asiae, Ascona, Switzerland.
ASI (NIS)	Archaeological Survey of India, New Imperial Series.
AT	Art and Thought.
BASOR	Bulletin of the American Society of Oriental Research, New Haven.
BBM	Bulletin of the Baroda Museum.
BDCRI	Bulletin of the Deccan College Research Institute, Poona.
BEFEO	Bulletin de l'Ecole Française d'Extrême-Orient, Paris.
Bhāratī	Bhāratī, Bulletin of the College of Indology, Varanasi (formerly Banares).
BITC	Bulletin of the Institute of Traditional Cultures, Madras.
BMFA	Bulletin of the Museum of Fine Arts, Boston.
BMQ	British Museum Quarterly, London.
BO	Bibliotheca Orientalis, Leiden.
BPWM	Bulletin of the Prince of Wales Museum, Bombay.

BSOAS	Bulletin of the School of Oriental and African Studies, London.
BV	Bhāratīya Vidyā, Bombay.
CF	Cultural Forum, New Delhi.
CR	Calcutta Review, Calcutta.
CS	Current Science, Bangalore.
EA	The Eastern Anthropologist, Lucknow.
EW	East and West, Rome.
Expedition	Expedition, Bulletin of the University Museum, University of Pennsylvania.
FA	France-Asie/Asia, Tokyo.
FRP	Field Research Projects, Coconut Grove, Miami, Florida.
GJ	Geographical Journal, London.
GR	Geographical Review, New York.
HJAS	Harvard Journal of Asiatic Studies, Cambridge.
IAC	Indo-Asian Culture, New Delhi.
IAR	Indian Archaeology—A Review, New Delhi.
IC	Indian Culture, Calcutta.
ICAA	International Conference on Asian Archaeology, Summaries of Papers, New Delhi, December 1961.
ICO	International Congress of Orientalists.
IHC	Indian History Congress.
IHQ	The Indian Historical Quarterly, Calcutta.
IJHS	Indian Journal of the History of Sciences, New Delhi.
ILN	The Illustrated London News.
IMB	Indian Museum Bulletin, Calcutta.
Ind Ant	The Indian Antiquary, Bombay.
Ind Art Let	Indian Art and Letters, London.
Ind Ir	Indo-Iranica, Calcutta.
IPEK	"Ipek," Jahrbuch für Prähistorische und Ethnographische Kunst, Berlin.
ISCA	Proceedings of the Indian Science Congress Association. Abstracts of Papers.
ISPP	Indian Studies Past and Present, Calcutta.
IWI	Illustrated Weekly of India, Bombay.
JA	Journal Asiatique, Paris.
JAHRS	Journal of the Andhra Historical Research Society, Rajahmundry/Hyderabad.
JAIH	Journal of Ancient Indian History.
JAnSB	Journal of the Anthropological Society of Bombay.
JAOS	Journal of the American Oriental Society, New York, New Haven/Baltimore.
JARS	Journal of the Assam Research Society, Gauhati.
JASB	Journal of the Asiatic Society of Bengal, Calcutta.
JASP	Journal of the Asiatic Society of Pakistan, Dacca.
JBBRAS	Journal of the Bombay Branch of the Royal Asiatic Society, Bombay.
JBHS	Journal of the Bombay Historical Society.
JBHU	Journal of the Benaras Hindu University, Varanasi.
JBORS	Journal of the Bihar and Orissa Research Society, Patna.

JBRS	Journal of the Bihar Research Society, Patna.
JESHO	Journal of the Economic and Social History of the Orient, Leiden.
JGJRI	The Journal of the Gaṅgānāth Jhā Research Institute, Allahabad.
JGRS	Journal of the Gujarat Research Society, Bombay.
JHS	Journal of Haryana Studies, Kurukshetra.
JIH	Journal of Indian History, Trivandrum.
JISOA	Journal of Indian Society of Oriental Art, Calcutta/Baroda.
JMPIP	Journal of the Madhya Pradesh Itihāsa Parishad, Bhopal.
JMSUB	Journal of the M.S. University of Baroda.
JMU	Journal of Madras University.
JNES	Journal of Near Eastern Studies, Chicago.
JNSI	Journal of the Numismatic Society of India, Varanasi (formerly Benares).
JOIB	Journal of the Oriental Institute, Baroda.
JOR	Journal of Oriental Research, Madras.
JRAI	Journal of the Royal Anthropological Institute.
JRAS	Journal of the Royal Asiatic Society of Great Britain and Ireland, London.
JRASB	Journal of the Royal Asiatic Society, Bombay.
JRCAS	Journal of the Royal Central Asiatic Society, London.
JRSA	Journal of the Royal Society of Arts, London.
JSHS	Journal of the Sind Historical Society, Karachi.
JSOR	Journal of the Society of Oriental Research, New York.
JSVOI	Journal of the Sri Venkatesvara Oriental Institute, Tirupati.
JTS	Journal of Tamil Studies, Madras.
JUB	Journal of the University of Bombay.
JUPHS	Journal of the U.P. Historical Society, Allahabad/Lucknow.
KHR	The Karnatak Historical Review, Dharwar.
KUML	Århog for Jysk Arkaeologisk Selskab, Universitetsforlaget I Århuus, Denmark.
LK	Lalit Kalā, A Journal of Oriental Art, chiefly Indian, New Delhi.
Luzac's	Luzac's Oriental List and Book Review Quarterly, London.
Man	Man, London.
Marg	Marg, Bombay.
MASI	Memoirs of the Archaeological Survey of India, New Delhi.
MI	Man in India, Ranchi.
MIM	Memoirs of the Indian Museum, Calcutta.
MJL	The Museum's Journal, London.
MR	The Modern Review, Calcutta.
Museum	Museum, UNESCO, Paris.
NA	Novvi Vostok, Moscow.
Nature	Nature, London.
NIA	New Indian Antiquary.
NO	The New Orient, Prague.
NPP	Nāgarī Prachāriṇī Patrikā, Varanasi.
NR	The New Review, Calcutta.
Novitates	American Museum Novitates, New York.
OLZ	Orientalistische Literaturzeitung, Monatsschrift für die Wissenschaft vom ganzen Orient und

	seinen Beziehungen zu den angrenzenden Kulturkreisen, Berlin.
Or Art	Oriental Art, New Series, London.
PA	Pakistan Archaeology, Karachi.
PAPS	Proceedings of the American Philosophical Society, Philadelphia.
PASB	Proceedings of the Asiatic Society of Bengal, Calcutta.
PBA	Proceedings of the British Academy, London.
PIHC	Proceedings of the Indian History Congress.
PJ	Prācī Jyoti, Kurukshetra.
PPS	Proceedings of the Prehistoric Society, Cambridge, England.
PQ	Pakistan Quarterly, Karachi.
PUJ	Patna University Journal.
QJMS	Quarterly Journal of the Mythic Society, Bangalore.
QRHS	Quarterly Review of Historical Studies, Calcutta.
RAA	Revue des Arts Asiatiques, Paris.
Rev Arch	Revue Archéologique, Paris.
Researcher	The Researcher, A Bulletin of Rajasthan's Archaeology and Museums, Jaipur.
RL	Roopa-Lekhā, New Delhi.
S	Science
SA	Sovetskaya Arkheologiya, Moscow.
S Am	Scientific American, New York.
Saṁskriti	Saṁskriti, Dr. Ādityanāth Jhā Abhinandan Grantha, New Delhi, 1969. Three Vols.
SC	Science and Culture, Calcutta.
SE	Sovetskaya Etnografiya, Moscow.
SHSIA	Symposium on the History of Sciences in India. Abstracts of Papers, New Delhi, October 1968.
SIAR	Smithsonian Institution, Annual Report, Washington.
Syria	Syria, Paris.
TASSI	Transactions of the Archaeological Society of South India, Madras.
TC	Tamil Culture, Madras.
VBQ	The Viśva-Bhāratī Quarterly, Santiniketan.
VDI	Vestnik Drevnii Istorii, Moscow.
VIJ	Vishveshvaranand Indological Journal, Hoshiarpur.
VVRB	Vallabh Vidyanagar Research Bulletin, Anand.
ZB	Zentralblatt für Bibliotekswesen.
ZDMG	Zeitschrift der Deutschen Morgenländischen Gesellschaft, Wiesbaden.
ZE	Zeitschrift für Ethnologie, Braunschweig.

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